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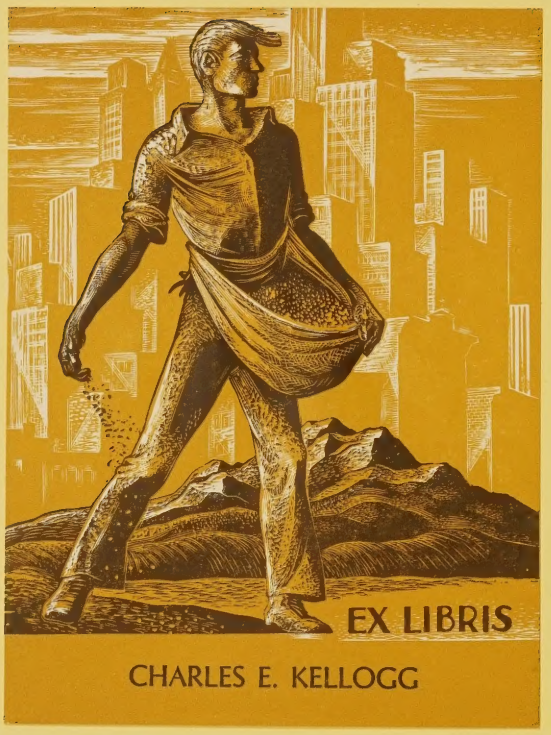
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CHARLES E. KELLOGG

C U R R I C U L U M V I T A E

Charles E. Kellogg

Volume 1

1902 - 1947

Preface

This curriculum vitae was conceived in the 1950's. For the earlier years it was constructed mainly from personal records of events, correspondence files, field diaries, and the memories of both Lucille and me. I had access to many of Dr. Marbut's papers and to Dr. H. G. Knight's large diary, which was helpful for dating in the 1930's.

Such sources were used in later years and, in addition, I kept a daily diary, supplemented by copies of a few key memos. Essentially nothing in my collected published papers is repeated.

The whole was dictated to Lucille and reviewed both before and after typing. But small errors in spelling and punctuation doubtless remain.

I wrote about events on the basis of facts known to me and the seemingly obvious intent of other people. Certainly I could not claim that my judgements were infallible in all details. A few of the people I worked with, in the universities, in the USDA, and overseas, lied to me and to others on numerous occasions. The most difficult were those with narrowly specialized training, without the basic subjects, and who were both uncertain of themselves and eager for money, rank, and power. They ran scared much of the time and tended to be increasingly arrogant, unhappy, and untruthful as they grew older. Many men took their religion for granted until they reached 60 or more. Then it occurred to them that possibly their only monuments would be in this world, yet they had none. Several men I've known in both universities and government became very bitter at this stage.

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Fortunately, I dealt with a far larger number of people who had no reason to lie to me. Some of these were plodders; others were able scholars, administrators, or both. Several hundreds of kind and able people helped me along the way.

The details of important overseas trips are covered in several large volumes. These journals for 1938 and 1945 are helpful but a better format was developed for the Alaska Journal in 1946 and used thereafter. Details of only short overseas visits are included here.

This curriculum vitae is a personal and confidential document that may not be quoted for publication without permission of myself, Lucille, or my son Robert prior to 1990.

The table of contents gives only a few key items for each year.

Table of Contents

The headings give only a few selected items. Some special abbreviations are used as follows:

ARI -- Agricultural Research Institute, NRC.

carto -- Cartographic Division of Soil Survey (in caps), or methods.

ISSS -- International Society of Soil Science.

land-grant -- Specifically, the agricultural colleges of the National Association of State Universities and Land Grant Colleges.

ldc's -- Less developed countries.

NCSS -- National Cooperative Soil Survey.

psc's -- Principal soil correlators.

NRC -- National Research Council.

NSF -- National Science Foundation.

reg -- Regional

RICOP (or Ricop) -- Resident Instruction Committee on Policy (land-grant group).

Soil Congress -- of ISSS.

SS -- Soil Survey.

SSSA -- Soil Science Society of America.

Study -- A Study of American Colleges of Agriculture, supported by a Carnegie Corporation grant -- 1962-1966.

1943 204-226

Maps for military; Color of desert soils; war crops; begin terrain intelligence; started garden; "two" secretaries of agr.; Mayo Clinic; Dr. Black rescues SS; Nygard joins SS; N.Y., Soviet Friendship Soc. and the "red hunt."

1944 227-243

Cooperative agents; naval fliers; fertilizer estimates; garden lectures; military edition of The soils that.....; Parker's dirty work; Drosdoff slighted; Post-war com.; Ill.; old Mich. surveys.

1945 244-273

Fertilizer guide; G.D.Smith; Auchter leaves; Cornell lecture; fertilizer controversy; father dies; Canada SS; first visit to USSR; World soil map; Wallace and Harriman; Brannan on TVA; Missouri Valley Authority proposal; FAO in Quebec; Ignatieff.

1946 274-298

SSSA threatened; W. M. Johnson to SS; world food potential; fertilizer bill fails; Jurion comes; Alaska; Mayo Clinic; Carto change; Atlanta on freight rates; Pacific soil surveys begin; Alaska hearings; Sigma Xi invitation; 2nd invitation to Moscow.

1947 299-300

Bennett in Science; Cline to Hawaii; Sigma Xi tour; debate on USSR; ISSS, Congo, Europe, etc.; Principal soil correlators; Efficient use.....; Bradfields curious pamphlet on fertilizers.

1948 231-355

NCSS conf. expands; tropical soils; Moon to TVA; silly talk of "land policy"; Heinen starts well; C. G. Stephens here; Conflicting doctrines....; Rothamsted, 2-8 July; Aubert's first visit; Claud Fly; The road to survival; food potentials; Black at Harvard; Salter and "Time"; The soils men live by; draft of Efficient Use.....; SS for UN; Brannan vs TVA; soil classification in "Soil Science"; Cooper Union.

1949 356-387

Australia and New Zealand; Soil conservation -- fun at Minneapolis; Memphis, NCSS; Food, soil, and people; Muir here; Black again; Tom Walsh; Iowa; Chamber of Commerce; Bromfield; SCS reviews Efficient Use...;Michigan; UN in NYC; Brannan and Bennett, Congo bul.; Riverside and Harper; "Post War Council"; SS 50 years old; Mayo Clinic; Arkansas.

1957 693-715

Williams against soil correlators; Ill., NCSS reg.; A lament for B; Mary Alice marries; Yearbook editor messes Yearbook; California; Cornell; Pochvovedenie translation begins; State soil sc. over senior soil correlators!; Montgomery comes; St. Louis, NCSS; Retzer comes; Edelman here; first kidney stone; Tavernier here with PSC's; with Nixon to MSU; Michigan visit; offer to FAO; handwriting analysis; threat of ulcer; more training; Tuscon, reg. conf.

1958 716-736

India, soil correlation attacked again; Yearbook collection; USDA library in trouble; Williams misquotes Peterson; rural development fails; Peterson fails me: more attack on soil correlation (Norton); 2nd Soviet Union trip: more pressure from FAO; Nevada.

1959 737-747

Rome, Iran, India, Viet Nam, etc. 15 Jan. to 30 April; NYC, Oppenheimer; Va. inspection; training at Cornell published soil surveys vindicated; prepare for 1960 Soil Congress in US; range-soil study; Utah problem; many poor state soil scientists; Iowa-Bottum and J. S. Mill.

1960 748-778

Iceland SS; Parker's stupid fertilizer curves; Lapham dies; "SID"; St. Louis, NCSS; D.Sc. Gembloux; Minn; Utah; Congo terror; 2nd kidney stone; Ligon's troubles; Soil Congress, Madison; SS directors GS-15, but not psc's; Williams to India; SCS-ASCS; Parker at old tricks; "Agronomy" Soc. building; Cornell.

1961 779-804

Miss. and La.; failed with ICA; Parker lies about world "soil map"; Ill. inspection; Bartelli to Ligon; failed on shelter belts; TVA handicapped; ARS plagarizes; Cochrane "gets" O. Wells and he goes to FAO; "carto research notes"; Norton slips; Young to Asia; Welch vs Williams; Oregon; Wyoming SS; Sec. of ISSS; FAO "map"; UNESCO "source" book; St. Louis; Nevada; Lincoln.

1962 805-823

Belgian Ac. Sc. Overseas; Muir dies; Cosmos Club row; Jackson of Carnegie; USDA gold medal for Smith; World Food Forum; Photo with JFK; D.Sc. NDSU;training; Bismarck, N.D.; Soviet guests; prepare for Geneva; more Soviet guests; N.Z. trip; JFK demands cash; Toronto; Study of agr. colleges begins at U. of Md.

1967 949-992

Dean Berg on Food and Fiber; satellites, etc.; arable soils of world; Williams renigs; New Orleans, NCSS; Carto; AID in Brazil; SS and computers; 1:1,000,000 soil map of US.; film on Ulysses; Hon. member Indian SSS; Lester Brown's nonsense; Moseman leaves AID; Army Engineers make bad maps; Carrol does well in Ivory Coast; Young meddles; Leo leaves D and R; Omer Kelley and Brady; Young retires!!; illegal contributions required; clandestine meeting of psc's; Cornell; Lucille to hospital; absurd Water for Peace conference; Robertson and Johnson; Miller and Arab nomads; land clearing at Peoria; got Chamber Music; SS lab study"; land classification for irrigation; Des Moines, SCSA; Our soils in books; Amsterdam and Paris; Jurion here; El Paso, Texas; Udall after FS and SCS; SSSA here, Brady and Cline; Columbus, land-grant.

1968 993-1076

Libya; publication delays; Riverside, reg. NCSS; Mary Alice' husband dies, Glen Cove; book exhibit; Washington riots; Yearbook proposal; Sean Cooney here; Mehren leaves; Kenneth Grant to come; Cady; Hon. member of Royal Soc. of N.Z.; plan for Johnson to Washington, with Robertson's help; Bulik; Bayley fails; Clemson, reg. NCSS; economic development; Williams makes work; L. Nelson to Columbia; Garland gripes; TV beats Humphrey; W. David Hooper lies; Brammer calls; Lincoln, state conservationists meeting poor; building new room; Congo; Williams to leave; Land-grant here, poor; Front Royal; Hardin to USDA.

1969 1077-1159

Williams leaves trouble; land-use policy; E. L. Peterson vs Oliver; Sir William Slater here; Moseman's bad paper; Charleston, NCSS; Grant and Smith on research; Fort Worth; Byerly ill treated; Rome on research in Africa, ruined by Devred; Batavia, N.Y.; Carroll to Kinshasi; Wisconsin inspection; Ferris State, NSF; Cady; Dean Cowden's very important visit; Williams had caused the trouble; bronze of Joyce comes; NAL doing badly; Simonson makes trouble; Robert back from Iceland; Hardin supports SS; Robertson on NAL; ARI, Brady to Buenos Aires, good; Simonson's papers; Memphis, NSF; SCS planning guide; PSC's here; Detroit, SSSA; US position of FAO; Charlottesville; Sewanee, NSF; Tavernier here; Johnson's competition; Grant pushes hard for van der Voet; Cowden and town-and-country planning; Truog dies.

1970 1160-1231

DIA drops W.S. Geography; Rourke to psc at Upper Darby; Ableiter retires; Grant agrees capability groupings poor; Brookings interview; NAL; Johnson to GS-16; Mobutu at Yangambi; NAL; "Conservation needs inventory"; Lexington (VMI), NSF; trouble in Minn.; errors in soil surveys;

1970 (continued) 1160-1231

USDA research on soils and nutritional quality of plants; Tinsley gripes on Carto; Virginia inspection; NAL advisory committee; Shem and Lucille have accident; AID conf. wasted; suggest retirement to Grant; Nat'l. Land Brokers on town-and-country; FAO Interagency Committee; Van Dersal asks for bad examples; Clemson on town-and-country planning; Grant and Hollis; Koechley retires; Walter Russell here; T-and-C for Yearbook; Cline comes to work on 3rd ed. of SS Manual; Grant gives report for comment; principles of planning; Agr. economists in USSR; FAO sends poor paper on their work; Greenland calls, then Jones, also of UK; treatment of Negroes in SCS; REA booklet; Civil Service to be raided again; SS trouble in Virginia; Soviet visitors; Portsmouth, state conservationists meeting; Quebec City; chez Ignatieff; Aubert, Tavernier, Moormann, et al chez Johnson; Soviet team chez nous; Nixon's "private contractors"; Mary Alice marries again; much work on 3rd ed. SS Manual; Leo on Lybia; SS "savings".

1971 (1 January through 1 July) 1232-1277

More house repairs; FAO Interagency; SS in Alaska; Charleston, NCSS; NAL; "Environment"; Grant and Hockensmith; Nixon attacks USDA; Delaware "last acre"; misuse of computers; "environment" and corruption; Yearbook, 1972; Cornell; Jurion here; Robertson leaves; 3rd ed. SS Manual; Johnson's clearance; Paul Lemmon badly treated; statements on civil service raid and USDA; politics; Senate hearings; "Environment"; answer attack on FS (p. 1261a); writing for FAO; Rourke to Liberia; uncertainty; plan Rome visit; rioting; bulletins to home; Oris Wells here; Johnson's appointment approved; May 28, last day in office; next day to Rome for week; big garden job; old office; Teakle here; July 1, retirement party.

Charles E. Kellogg: Curriculum Vitae

August 2, 1902. I was born on Grandfather Kellogg's farm in Ronald Township, Ionia County, Michigan. My parents were Herbert F. Kellogg (n October 2, 1879) and Eunice Irene (Stocken) Kellogg (n August 27, 1876), formerly near Galesburg, Michigan.

My earliest recollections are of (1) my parents quarreling, (2) outbursts of anger, periods of petulance and cruelty, and the occasional days of gaiety of my father, (3) many short periods of illness, (4) the dignity, kindness, and Victorian intellectualism of my paternal grandmother, Mary Elizabeth (Fausett) Kellogg, and (5) the practical farm housewife who was my maternal grandmother. She I saw only now and then when she and Mother's sister came to visit us and the uncommon visits we made to Galesburg.

In those early days Father usually had a hired man. With few exceptions, most worked only a short period before my father's anger or sharp criticisms drove them away.

The winters were snowy and cold. The house was cold, except very near the stove. At first the kitchen and dining area were together in a small room. On winter washdays, the

house was full of steam and one could hardly see out of the frosted windows.

1904(?) I was a bit over two years when my grandfather died of gangrene caused, basically, by diabetes. I have only the recollection of standing by his chair when he was ill. From all accounts that came to me later, he was a wise and good man, widely known and trusted. For many years he was supervisor of Ronald Township, a member of the Ionia County Board, and superintendent of the Methodist Sunday School in our village, Palo, some three miles northeast of the farm.

My father had a flair for chemistry and electricity as a boy. Many of his chemicals and other small gadgets were still to be seen in the basement of my grandmother's house, some 200 yards from ours. (The shops were back of her house and the barns between the houses, and a bit farther from the road.) For some reason he quit high school in Ionia a bit before graduating. As an older boy I was told many tales about escapades and serious "practical jokes",

name. Father was putting back farm income into its improvement,

that he and William Bennet, Ben Steer, and others carried out in the community. Apparently my grandfather let him do as he pleased and he never learned how to work with people. He took a job in Lansing as an electrical engineer and he and Mother lived there for a very short time. According to my father's story, Grandfather Kellogg insisted that he return to the farm; and the house I lived in was built for him. Although I never heard any other story, I doubt that this was all of the reason why he came back to the farm.

1905. By far my fondest recollections are of Grandmother Kellogg. I loved to stay in her house. My memory fails to go back before she read to me. As the years went by she read to me out of many of the great classics. (Although later I read at home the usual children's books and boy's adventure stories.)

As time went on I realized that my mother and grandmother were not fond of one another. I could see later why this was so. My mother had not finished high school, while my grandmother had been much more broadly educated and was very well read indeed. Further, the farm was entirely in my grandmother's name. Father was putting back farm income into its improvement,

so had he died, my mother would have been penniless.

1906. In summer, threshing was a big event. Many neighbors came to help under the custom of exchanging men and teams for this work. And my mother always gave them much to eat. Extra men were also hired for the harvest of hay, grain, beans, apples, and other crops. In autumn pigs were butchered and marketed. Hams, bacon, and salt pork were put away for home use.

On the other side of Grandmother's house was the garden, prepared many years ago by my grandfather when he first came to this place from Newfield, Tompkins County, New York. Very early I was given chores to do in the garden, which gradually increased over the years. This work I liked, but it eventually took much of the daylight time remaining after other work.

My mother insisted in letting my hair grow into great curls that hung down to my shoulders. (She would have been better pleased with a daughter). But in 1906 I had my first proper haircut. (And Mother saved the curls!)

1907. In September I began school at Ronald Center School. Ula Little, of my same age and a bit north of us, also began.

I had to walk only one-half mile south to the school. My first teacher was Laura Cole. She had about 13 pupils for 8 grades in one room. The library had about 5 books. (Later I read two of these many times - children's stories of the Trojan War and of King Arthur and his Knights.)

In the evening after my first day in school I fell down the cellar stairs and cut a huge gash in the back of my head. The deep scar remains.

1907-9.

After 1906 life went on between school and farm work with rare visits to Galesburg and more frequent ones to Ionia, and especially to Palo. How I loved the rare chances to stay a few days with my maternal grandmother's sister and her husband, Aunt Em and Uncle Dan Cheney, in Ionia. Then their son had four daughters, roughly my age.

Some days my father could be gay and very interesting. He had many skills. I cannot remember when I learned to use ordinary tools for wood and metal working. At other times he would be very bossy and irritating. He hated labor unions. He admired wealthy people.

He was said to have been slender as a young man. But I recollect him as overweight, in fact quite fat - about 240 pounds. He ate a great deal of sweets, especially just before bedtime. He loved peanuts. All of his right-hand suit pockets were a bit greasy from carrying bags of salted peanuts.

While on a visit with us, word came to my Grandmother Stocken that her son, Ray, my mother's brother, had been killed in an accident while working as a carpenter constructing a dam. Never have I seen a greater display of grief and despair.

1910. My father added onto the house about this time. He built a large kitchen and a "back room" for storage downstairs, with a bedroom over the kitchen for me. I liked a room for myself, especially after I got a little stove. It had a slanting roof without an attic above, and was exposed on three sides. So it was very cold in winter and hot in summer.

1912. In autumn my father got his first car - a 1912 Model-T Ford. The first day he drove it, he sprained his wrist cranking it. (Oh the days and weeks he was to spend playing

with that car while I was left with the chores! He took it apart and put it together countless times.)

1913-1914. During this period I became very depressed. Every idea or hope was ridiculed by my father. He repeated again and again how stupid and heedless he thought me to be. Finally I began to believe it, and seriously considered suicide. Happily, I began to read Ralph Waldo Emerson about this time; His essay Self Reliance snapped me out of this mood. After that my father was a serious problem to be reckoned with but the terror left me.

Later I considered leaving home completely and find a place to work and go to high school. But my mother would have been broken.

Not much happened that I recall. About 1914 I fell in love with a new girl at Ronald Center, but it didn't last long.

We would go to Grange about once or twice a month; Grandmother, Mother, and I went to the Methodist Church in Palo about 2 or 3 times a month; and, in winter, about once or twice a month we went to the Ronald Literary Hall, where the local folks put on plays. (Later I had a part in one or two).

I took my first girl to a play there the winter of 1914-15 in a cutter.

1915-1913. In the late spring all of the eighth grade rural-school children gathered at the Ionia Court House for examinations. These took 2 or 3 days as I recall. I staid with Aunt Em and Uncle Dan. These were quite a strain for a farm boy from a tiny school (some years we were only 3 or 4) who had never had a real examination before. But somehow I passed and we all listened a few weeks later to a speech and got our diplomas. All I remember of the speech was that we would be more proud of that 8th grade diploma than any we might get in the future. But that did not turn out to be true.

Summer work and then to Palo High School in September. Part of the time I rode a bicycle but most of the time I drove a horse. One year I had a stubborn Shetland Pony for his keep.

In very bad winter weather, I had a wonderful place to stay - with Miss Fanny Edwards and her older shy sister. Fanny had been the housekeeper for a Mr. Mandeville, partner

with my Uncle Henry Pew in his general store, including drugs and a bank. When Mandeville died he left his property for the use of Fanny Edwards. He had accumulated a nice library of serious books. So when I staid there I had no chores but a fine library and two very kind people.

But most days I had chores to do both morning and evening. This left little or no time for sports. I was small for my age then and might not have been able to develop talent but I would liked to have been able to play basketball and tennis. All I did was to take part with a debating team a few times. Our high school had only some 30 or 40 students in the top four grades so we had few planned debates or literary activities.

We had two teachers the first year. Both were fair to good, but as time went on the new teachers were continually poorer. The last year neither was good at all.

1918. In the autumn of my senior year in high school, the First World War ended the first day I had long pants. My clothes were a problem then. My mother "made over" an old black suit of my uncle Ray's as I recall, some 15 years out of date.

But my biggest embarrassment was lack of money. Although I worked very hard on the farm, my father never gave me money unless I asked for it, which I was extremely reluctant to do. My grandmother gave me a little. I earned a bit by trapping muskrats and skunks, and, on Sundays, by gleaning stray pods of white field beans after harvest. (Beans had a very high price during the War.)

1919. During the winter I had a severe attack of the war-time influenza that killed so many soldiers. It seems a miracle that I survived. For over a week I was unconscious. I missed a great deal of school but managed to keep my top standing among seven - the largest class in the history of Palo High School!

Because of that honor I was permitted to stay many nights with Uncle Henry and Aunt Hetty Pew. They had a typewriter and I wrote there my first speech. (And it was printed in the Palo Post!) Both Aunt Hetty and Uncle Henry were very nice to me. For one thing they always talked <u>to</u> me never <u>down</u> to me. Uncle Henry used to sell me the regular 25-cent Macmillan classics at cost and thus helped me to build

a good library during high school.

But then in May I got the measles, which hit me very hard since I had scarcely recovered from the influenza. I had some concealed blotches on my chest at graduation.

The worst possible catastrophe happened the late spring and early summer of 1919: Grandmother Kellogg had a severe stroke and never fully recovered mentally. She had promised me help for college. But then father took over her affairs.

He promised me that if I would stay home on the farm for a year he would send me to college for a year. I planned to get a teacher's certificate and then go to law school later.

Directly after high school I had a very severe attack of asthma, which had always bothered me, especially after exposure to grain dust. The old family physician found, as a last resort, that whiskey would knock it out. So I had my first drink of whiskey and my first 24-hour sleep after a horrible week of sitting in a chair laboring to breathe.

Then my father farmed me out to work for a highway contractor developing a new graded and gravel-surfaced road by our farm for some 5 or 6 miles. This was a rough job for

a boy weighing only 110 pounds. I drove a team on the old-fashioned wheel scraper with the following schedule: (1) chores, harness my team, and be at the job at 7:00 a.m.; (2) work with the wheel scraper for 10 hours, not counting an hour for lunch and care of the horses at noon; and (3) leave the job, return home, take care of the horses and do other chores. And six days a week! I was continually exhausted for the six or eight weeks I worked. And my father got the money except for about a week I worked as a laborer without the team.

This was the year I was able to put a stop to father's customary mad rushes to strike me down. We were moving some machinery in the yard. Suddenly something, I never knew what, angered him and he started for me. I picked up a broken whipple tree and offered to kill him if he came closer. He didn't ever again. But this didn't stop his continual complaining.

This was mainly a wasted year. I went out very little, but I did get some reading done. My folks urged me to go out more. My mother used to tell me that no nice girl would like me. (She couldn't have been more wrong.)

Yet gloriously, I met Lucille Reasoner in the Autumn.

Her father had come to be principal of the Palo High School - and a splendid man he was, yet most too quiet. It took Lucille and me a year or so to get acquainted

A bit in previous years, but especially in the winter of 1919-20, I went many times to Ionia with team-drawn loads of grain or other products and returned with coal and other supplies. It was ten miles each way and the team had to walk. I had a lantern on the wagon tongue in front of the horses at the start and for the last few miles home.

1920. In the Spring I had an accident. While pruning in the apple orchard, I started to fall from a high tree. I grasped a limb on the way down, but this wrenched my back. I had to make several trips to an osteopath, which irritated my father no end. But he let me drive the car a bit.

In the summer I saw Lucille quite a bit. Fortunately both my father and mother liked her. I worked full hours on the farm until September. Then the matter of college was brought up. My father would have no part of it, regardless of his agreement. But he did say he would send me for the

2-year, 16-week short course for farm boys at the Michigan Agricultural College. I would have taken <u>any</u> course to get away and get started in college. My father spoke vaguely of partnership.

So in October I went to East Lansing. I had a small room with another boy whose name I no longer remember. I enjoyed the courses and most of the professors. They urged me to take a degree.

At Christmas time my father complained about costs, as I had expected. Fortunately, I was able to give him a detailed accounting of every cent. I saw Lucille again and we became even better acquainted.

1921. After another six weeks at MAC I came home near the end of February and worked on the farm. We had a little Model-T pick-up that I could drive. With effort, I could take off the box and put on the regular trunk to make it a run-about. Lucille and I were out quite a bit in this car - "Blub" we called it. This summer, when I was just turned 19 and she 16, we definitely planned to marry.

But still I had no regular income from farm work. It was plain how the "partnership" would work out: I would do the work and Father would give the orders, run around in his car, and have the money, including all of my grandmother's.

So I decided to return to college for a degree. Lucille agreed absolutely. I told my father one night in early September. He became angry and told me he would give me no money (Actually he did, but only a few dollars.)

I had saved a bit of what I had had from sheep, from trapping, and from my grandmother earlier. My mother gave me a few dollars and did my laundry, which went back and forth by mail in a canvass-covered box. She also got me a sheep skin coat during a trip to Ben Steere's store. Steere had been primed by letter to talk me out of college, but it was no use.

October 1921. Thus I started the fall term as a freshman. I took a few extra credits in the hope of finishing in three years. I took exams for credit for the work done the previous year

as a short-course student.

I lived in a small room with a boy called Jackson. Happily, I got a job in the college greenhouse. We had to take military and I took cavalry, which was fun.

I was home for Christmas and saw much of my girl. My poor old grandmother offered me money - two hundred dollars! She started to rise to get it but I (foolishly) said, "You don't need to hurry now." She sat down. But we had been overheard. When she went to get it later, she couldn't find it!

1922. Early in January I was back in school with the greenhouse job and also worked for my board in the student dining room. I accepted an invitation to the Dorian Literary Society - a social fraternity that later became Phi Kappa Tau. Otherwise I lived very cheaply indeed.

An old family physician, Dr. Charles B. Gauss of Lansing, called to say my father was extremely ill. I went to Ionia with him and on to Grand Rapids, where my father was taken. He had a terribly painful kidney infection. I came back soon to East Lansing. A little later he had a successful operation

to remove it, following which he had diabetes the rest of his life.

Since we had a few parties, Galloway, a fellow student my same size, and I bought a tuxedo together.

Early in March my best girl came to East Lansing, and together we went to the Dorian winter formal, our first dress-up party.

Then in March my father came home from the hospital and I had to quit school and run the farm. The hope to finish in three years was out. By April Father was well enough too drive his car about but not to work. But he could boss and complain lustily.

In May, I gave my second speech - one of welcome to the usual homecoming crowd for Decoration Day at Palo. (I recall that my legs trembled.)

Lucille and I had a wonderful companionship and when she graduated from high school I gave her my Dorian pin.

September 22 Lucille went to Kalamazoo Normal and a bit later I returned to East Lansing with little money. My father

gave me $30 per month (about one-half the going wage for the work I had done) minus any money I had had.

This time, I went to live in the Dorian house, worked for my meals, and some at the green house.

I made two short visits home when Lucille did and "thumbed" my way once to Kalamazoo to see her. And we had a nice Christmas period except that I worked most of the time.

1923. We both returned to our respective colleges January 2, and I saw her in Jackson four weeks later.

By now I was out of funds and took a job in a drug store. It paid all of my costs and a trifle more. Another boy and I worked together. Generally, one week I would get to the store about 4:30 a.m., clean it and have it ready to open about 7 a.m. Then I would check the furnace at noon. The next week I would come to the store at 3 p.m. and work as a "soda jerker" until 10:30 or 11:00 p.m. We divided the work on weekends or doubled up so one could be away. It was a horrible grind. I was continually tired and sleepy. But there was no alternative. I kept this up until about a week

before the exams of the last term, during which I slept and studied. Somehow I passed everything with at least a "C".

I had two trips home when Lucille was there and one in Kalamazoo, and she came once to East Lansing just as I quit the drug store.

Early I applied for every job I learned of to earn more money in summer than I could hope to have on the farm. I would have taken any one although I preferred something in horticulture. I had had beginning soil science and didn't much care for it. Yet I was offered a job on a soil survey in Ogemaw County being managed by the State Departemnt of Conservation. A few days after I had agreed to go for $80 per month and my expenses, I was offered a job as fruit inspector on the Pennsylvania Railway . But it was too late!

Lucille and I were home for 3 or 4 days in June. I returned to Lansing and arrived on the job in West Branch, June 20, for the soil survey of Ogemaw County.
The first night the mosquitoes were terrible.

But I learned to live in a tent, learned to find my way in the woods, and gradually learned about soils and how to

1 9 2 3

Soil surveying was more of an art than a science compared to what it became later. We had no manuals and were supposed to learn how to do it by observing the more senior men. We had no standard vocabulary. All sorts of bizarre words were thought of on the spot for describing soil horizons. Many of the color terms used were especially confusing, such as chestnut brown, brick red, lemon yellow, chocolate brown, salt-and-pepper, and mouse gray. I have heard older men debate whether the name "chestnut soils" referred to the color of the nuts or to that of the bark of the tree. Consistence terms were just as bad. In conversation Schoenneman used to speak of silts as having a "schoolmam-leg field.

About this time C. H. Spurway at Michigan State was developing a test for soil acidity, which later became well known under the name "Soiltex." He had asked Professor Veatch to try it out in the field. He gave him some waxed papers, about half the size of cigaret papers, and some of the mixed dyes used in colorimetric determinations of pH.

I recall one day that Veatch, Schoenneman, and I were looking at a soil profile and Veatch decided to try Spurway's test. One creased the paper lengthwise, put a little spot of the soil about the size of a pea in the middle, wet it with the solution, and allowed the solution and soil to come into equilibrium. Spurway also furnished a little card showing the resulting colors on the

lefthand side and adjective descriptions of the acidity to the right. These ranged from very strongly acid to neutral and over to alkaline. Of course, we had there mainly acid soils. I picked up this card to look at it and noticed a column at the extreme right headed "pH" with numbers opposite each of the adjective descriptions of the acidity or alkalinity. Although I had sat through the sophomore course in soils at Michigan State, I had not had the term "pH." So I asked Veatch, "What does this column mean and the numbers under it?" He replied, "I don't know. It has something to do with chemistry." I am not telling this story in any spirit of downgrading either Mr. Schoenemann or Professor Veatch. Both of them were well above average senior staff men in the Soil Survey. But over the years since then I have recalled this incident in relation to what a junior soil scientist was supposed to be able to do by 1960. By then he had an enormous vocabulary handed him with much of it quite rigidly defined. In each 10-year period between 1920 and 1960 the beginning soil scientists in the Soil Survey had two or three times as much to learn immediately as those who started 10 years earlier.

map them under a wonderfully kind and able supervisor - L. R. Schoenmann, who became a great friend.

insert 20a and 20b

In mid-August we moved camp to Antrim County. By now I was becoming deeply interested in what I was doing. I had seen so many unsuspecting people sold poor soil. I had been in the nearly new but abandoned houses after failure. "Someone," I thought "must do something about this, and a soil survey will be needed."

Just before going home, I asked Schoenmann to tell me honestly whether he thought I had any talent for the work I was doing. He replied, "I think so, but I didn't want to influence you to change from horticulture."

So I left for college determined to become, if possible, a soil scientist. I stopped home and spent some time with Lucille and went on to school near the end of September. I had physical chemistry, geology, and the like. Because of Schoenmann's report on my work to the head of the Department of Soils, I got work in that department.

A few weekends and the Christmas holidays with Lucille.

1924. I had the hardest, most interesting, and best taught course I had yet taken - physical chemistry from Prof. Paul Hartsuck. At the same time I had a very dull course, called "Agricultural Chemistry".

In the winter term I organized a dining club at the fraternity house for my meals. Also I headed our rather strict scholarship committee. I also wrote for the Holcad - the campus paper - from time to time.

This term Lucille came for the big Junior Hop and Military Ball.

In addition to the club, I also worked some in the Soils Department, did chemical analyses in Horticulture, and worked a bit at a shoe store. I also was able to borrow $100.00 from the Knights Templar student loan fund.

When the Dorian Literary Society became the Alpha Alpha Chapter of Phi Kappa Tau, we had a big "do" and Lucille came to that. I developed a little fraternity "magazine" - the "Di Alphian" - and wrote for the campus paper.

I was home for a few days in June but had to go to Camp Custer for officer's training. Then near the end of July I joined the soil survey party with Schoenmann in Roscommon County at $90 per month plus my expenses. Jim Porter and J. W. Moon were already there. Early in September the party moved to Alpena County. Meanwhile Lucille went to her teaching position in Kalamazoo. I got a bit extra for working on holidays and had permission to register late at college.

I stopped home on the way to college. Since I had borrowed a bit of money, and planned to borrow a bit more my father demanded that I take some insurance to "my estate" to cover the debts should I die. One of his cronies was there to sell it to me. This cost me about $40. (It was a poor company that went into receivership in the early 1930's. But I did not lose much.) What cooperation!

I saw Lucille briefly and went on to college. We arranged to meet in East Lansing, Palo, or Kalamazoo quite frequently, although it did not seem so then.

I worked in the Department of Soils and in the greenhouse

When I landed in camp the only vacancy was a cot in a tent with Prof. J. O. Veatch. He and I had not hit it off well the previous summer and he began right away to ride me a bit. One day he criticized one of my maps and it was just too much. I said, "Prof. Veatch if you don't like it you go map it yourself. The land is all out there with no pool tables in your way." Veatch was one of those kind of fellows that would push you until he finally got a reaction. Always after that he supported me. Of course, he would criticize me; but he wouldn't let anybody else do it. (As some of the SCS boys found out the hard way later.)

But I borrowed a bit on a note at an Ionia bank and got a $200 loan from the Knights Templar. I was determined to have full time for study and not spend so much time working. Then too, I had a large group of students to look after in the cavalry. The lazy regular Army officer rarely showed and I had to train them to ride, to march, and to shoot. Yet this was good training for me.

Just before Christmas I joined the Masons in Palo. I worked on maps for pay during the Christmas holidays at home and saw much of Lucille.

1925. Lucille returned to Kalamazoo and I to East Lansing after New Years Day. I continued to work on maps for the Department of Soils and tutored in physical chemistry.

I went to Kalamazoo now and then and Lucille to East Lansing. Between terms we were both back home. In this winter term I was initiated into Alpha Zeta.

In the spring term, I was able to take two courses in physical chemistry toward graduate credit. The Michigan Agricultural College became Michigan State College - and later Michigan State University.

June 22, I received the B.S. degree. Lucille came to the graduation and gave me some $150 or so to pay up all my bills. We picked out a tiny apartment to live in next winter. Then I went to Menominee, Michigan to begin another summer on the soil survey at $150 per month. Schoenmann was there and Joe Moon.

We had a mixed crew. The worst was Miles Beck from Texas. Then Dacknowski-Stokes spent several weeks in camp. He told me that if I would make some special maps for him, he would get me a manuscript copy of Dr. Marbut's translation of Glinka. I did the work; but I'm sure he never intended to give me the manuscript. (Later I turned up as his boss!)

Near the end of September we moved camp over to Chippewa County near Sault Ste. Marie. It was really too cold and snowy, but the people had been promised a soil survey and Schoenmann wanted to make a start. We had a stove in the tent and I could start the fire from my cot.

Near the end of October I drove the little Ford, I had driven up north - last spring I traded a typewriter that I

had bought second hand as a freshman for it - to Palo and on to East Lansing.

Dr. McCool sent me over to Kent County and loaned me some money to see Lucille, who was there at a teacher's meeting. I bought her a little diamond.

Not much could be done there with such bad weather and Dr. McCool sent me down to Washtenaw County to map land use in detail over Wheeting's soil map (which was not entirely accurate back from the roads.) It soon got too bad for field work and I returned to East Lansing near the middle of November. J. W. Moon used my apartment for an office while writing the soil survey report of Menominee County. I was in Palo for Thanksgiving with my bride to be and back to East Lansing and worked in the soils lab until a day or so before Christmas.

Lucille and I were married at her home in Palo at high noon Christmas Day and went immediately to our first home in the little apartment on East Grand River Avenue in East Lansing.

1926. After New Years Lucille had to go back to her school in Kalamazoo. I worked in the laboratory and took a course in electro-chemistry. Lucille came over weekends but quit her job and came to stay about February 1.

Schoenmann recommended me for a position at Purdue University. After some correspondence I agreed to go about April or May. In the meantime Dr. McCool was negotiating for a fellowship from the State Highway Department for a Ph. D. candidate. In March, Mr. Burton, Director of Research, called at the college and Dr. McCool asked me in. Burton offered $1,200 with 2 terms at the College and the rest in the field and full time toward my degree. I said, "This is very generous but I cannot quite make it less than $1,500". He agreed (and a few months later raised it to $1,623). This made skimpy living for us, but it was too good a chance to give up. (Those were 1926 dollars!)

In the Highway Department I had the title of Research Engineer. I worked partly by myself, with an engineer and crew to paint the station numbers on the pavement, and partly

with Olaf Stockstad.

This was very interesting and rewarding work. I was in and out of East Lansing from April to October and at the College from October to March. ¶ In the lab I studied the effects of freezing and the like. Insert 27a

Burton was tough but I liked him. Lucille went with me on some trips and spent some time in Palo. ¶ When college opened in late September, 1926 ~~she~~ registered to complete her degree. So in the Fall and Winter terms we both had studying to do. We were poor but had a wonderful life together.

My course work at the College was excellent - advanced physical chemistry - insert 27a and 27b from Prof. Ewing and especially plant physiology from Prof. John Crist - one of the very best professors I ever had.

1927. I continued on this course. In the summer of 1927 I worked in the northern part of the State and Lucille went with me a good deal. ¶ That summer I studied the first newly planned road, from Cheboygan to Mackinaw City. All the earlier studies had been of existing pavements. On the basis of my research (and arguments) the designs were drastically altered, for the first time in the history of soil science, on the basis of a detailed soil

I had special equipment prepared that I standardized myself to determine the expansion of moist soils upon freezing. I ran determinations on a wide variety of soils at different moisture contents and found that the expansion was only a tiny fraction of what we were finding under the pavements. Then I did a little calculating which showed that if the subgrades were all water and froze to one meter, and if all the expansion were vertical, it would amount to only 10 centimeters. Thus it became perfectly clear that the frost action was due to the formation of ice lenses in place. As the ice crystal formed water was added to it, most commonly by capillary action, partly by seepage from higher land, and a bit by the movement of water vapor.

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We made very important progress with the study of existing cement concrete highways. The soils were mapped in great detail - about 25 to 50 inches to the mile. In addition, we indicated the horizon of the soil on which the pavement rested, including many nonconforming layers beneath. Frost action was one of the biggest causes of severe damage. We made special investigations and deep borings wherever there was serious touble and wherever we expected serious trouble and did not get it.

I collected hundreds of soil samples by horizons for engineering tests in the laboratory at the University of Michigan. Since we could map so accurately - we were really mapping soil individuals - and could supplement the official names with any necessary modifications, the laboratory results were nearly uniform for all genetic horizons. For example, tests on the B horizon of the Miami silt loam and the Miami loam soon became unnecessary. And the same became true of many of the other prominent soils. In fact, the correlations were better than we had anticipated.

Mr. Burton thought that we could give each soil description a file number and predict exactly how it would behave. I told him that I did not think this would be possible because of differences in water coming from the outside and because of nonconforming layers. He said, "You are implying that soils engineers will need authority to change the designs after the grades are opened up?" I replied, "This is true. For example,

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under the Miami at several feet the only way to predict whether there is gravel and sand, or heavy clayey Illinoian till would be by very expensive well-drilling equipment. Yet when the grades are opened up one can see what subgrade materials are needed. Otherwise money would be wasted by putting crushed stone with bleeders in sand or the pavement ruined by not doing so in the clayey Illinoian till.

Mr. Burton thought that this authority could not be delegated to the soils engineers. But it was and has been standard operating procedure in Michigan for many, many years.

The deep peat deposits were another great source of trouble. These had to be investigated thoroughly and carefully sounded with sample tubes to be sure when we had struck the mineral bottom. On old roads this mean well-drilling outfits to make holes through the earthy overburden. With this procedure we could make accurate cross sections and longitudinal sections and calculate the amount of fill required. This was piled up on top of the old roadway and dynamite charges placed every 10 feet on each side to sink it.

[illegible]

survey (at a scale of 25 inches equals one mile.)

But I could begin to see trouble ahead. Dr. McCool and Mr. Burton had clashing personalities. What one wanted me to do for a thesis the other would not accept. I pushed to get all the residence and course work finished for a Ph. D.

1928. In the winter I wrote my first real paper. Soil type as a factor in highway construction in Michigan. (Michigan Academy of Science. 10: 169-177, 1929.) I saw I would need to make a change. Again Schoenmann helped me out and recommended me to Prof. A. R. Whitson, Head of the Soils Department, University of Wisconsin to look after the soils part of a land economic survey modeled after Schoenmann's. Dr. McCool agreed to accept my research there for a Ph. D. thesis and Prof. Whitson agreed to let me do one.

I left my young wife, slightly enceinte, to finish her degree and arrived at Madison about April 15. I've forgotten the exact salary, but I believe about $2,500. People were very friendly. I staid there for a couple of weeks, or less, and then went to Bayfield County to make a soil survey.

The organization was confused. The survey was cooperative between the Geological and Natural History Survey of the University and the State Department of Conservation. The man in charge for them was a very foolish one named Borden. Mr. ~~Gieb~~ Geib - paid by the U. S. Bureau of Chemistry and Soils but located with Prof. Whitson - was nearly helpless with sleeping sickness. Yet nominally he was in charge of the soils work. But it was made clear to me by Whitson and ~~Gieb~~ Geib that I was to be actually in charge. Then also Whitson sent his crazy boy along, who should have been, and subsequently was, put into an institution. Still I had a good crew and we managed quite well.

The soil classification was not especially difficult. Prof. Whitson suggested that we map stoniness and I worked out the first scheme that I know about. This is in general use today. At this time Prof. Whitson was much opposed to Dr. Marbut, who was greatly admired by all soil scientists of my age. On a visit in early summer I gave Prof. Whitson our descriptive legend. After reading it he looked up slowly

and said: "I see you have the Bureau concept of soils." It was like telling a man he had leprosy!

Yet I learned much from Prof. Whitson. He emphasized the economics of the farm unit as a whole in selecting from alternatives for each field.

Then too, Dr. Marbut did mislead me and others with his continual emphasis on classifying and mapping soils on the basis of their profiles. Slope, temperature, and moisture were not even regarded as soil characteristics. I continued in this error for some time. It was not until quite a bit later that I saw clearly that we were both classifying and mapping (where the scale was large enough) soil individuals. In the beginning of this concept I wrote of "soil landscapes." Many serious errors in Marbut's work resulted from his failure to conceive clearly the soil individual.

In late August I returned to the University and went home. Lucille's parents had moved to a farm and the early morning after I arrived, our son Robert was born, September 2.

I was the physician's helper. After 2 weeks I returned to the Univeristy and found an apartment to occupy in the autumn. Then back to the survey. Finally, the Whitson boy ran away.

The survey got quite a bit of attention. In purpose, it was like that of the Land Economic Survey run by Schoenmann in Michigan. Prof. George Wehrwein, the well-known land economist of the University worked with us. We had a two-day visit from the governor of the state, who went out in the field with me all one day. He got soaked and I lent him dry clothing for the evening.

Winter came a bit early and we didn't quite finish. We left two townships that had been "mapped" the year before in very bad shape.

Later I prepared a report, published by the State, with Prof. Whitson's editing. The survey was reworked by J. K. Ableiter and much later, in 1961, it was published as a reconnaissance soil survey by the U. S. Department of Agriculture.

I came back to the University with samples of a large

number of characteristic soils. The furniture, Lucille, Robert, and Lucille's mother came about November 10. I had map work to do and started my work in the lab for my thesis on the samples.

The atmosphere of research and scholarship at Wisconsin was far superior to that of Michigan State. And how I loved the library! Although Prof. Truog and Prof. Whitson were feuding, I managed to stay on good terms with both, for a while.

1929. During January and February Prof. Whitson thought I should have only one-half time salary since I was spending all of my time in the lab. I worked from about 8 to 6 in the lab, and in the evening studied French and German and wrote on the thesis. This winter, I had my first sharp hip pains - the beginning of arthritis - but I didn't know it then. In the spring I finished the thesis, sent it to Dr. McCool, passed off German and French (Michigan State accepted slips from the University of Wisconsin), and studied for my Ph. D. examination.

Then a crisis arose. The graduate committee at Michigan State demanded a printed copy of the thesis. Professor Whitson had already asked me to continue and broaden the study to the whole state. But I had a hunch not to put off getting my degree. So I got estimates for special printing of 100 copies. Then Michigan State changed the decision: they would accept a letter from Prof. Whitson saying that it would be published later by the University, which he wrote to Dr. McCool. So they accepted three bound manuscript copies.

About the middle of June we sublet our apartment and went to East Lansing. I took my oral examination and visited old friends. So on June 24, 1929 I had the long-sought diploma. Everyone was very kind. The graduate dean told me that I had to put up a one hundred-dollar deposit that I would furnish copies of the published thesis. But, I told him, "I don't have $100." "You have a checking account don't you" he asked. I said, "Yes but I haven't that much in my account". "Do not worry", he said, "just write the check I'll never cash it". Which I did.

I returned to Madison. Prof. Whitson sent me up to Vilas County to straighten out Ableiter on the textural classes. He didn't check the old soil map. Finally I convinced him that Ableiter was right and the old map wrong.

While at Madison, I slept on a cot in one of the vacant offices in the Soils Building. On July 5, Mr. Geib and I began a trip over much of the state and I took samples of the principal soils. By about the middle of August, I had most of this done and began the laboratory work.

It was now becoming evident that the Wisconsin Soil Survey was essentially finished. Happily Prof. Truog had become very cooperative. He said he could get me a job with a fertilizer company at $5,000 or as Assistant Professor at North Dakota at $2,500 for nine months. I chose the latter and he recommended me to Dr. H. L. Walster.

This meant that I had to have the laboratory work finished by the end of December.

In the middle of November I attended the Soil Survey and Agronomy meetings in Chicago and saw many of my famous seniors

for the first time.

Dr. Walster, Dean of Agriculture at the North Dakota Agricultural College (now State University), and I had a talk in Chicago. He said the job was temporary and might become permanent. He said, "You will have only one class to teach - the beginning course in soils that runs through 2 terms." He agreed to find a furnished house for us if possible. A Mrs. Alva Benton was leaving about January 10 and he thought we could rent it furnished. ~~His~~ Dr Benton's brother would need one room and would take care of the furnace.

In the weeks left I gave a series of evening seminars to Prof. Truog's graduate students on soil morphology and genesis.

My parents visited us at Thanksgiving time and Father insisted on driving me, Ableiter, and, I believe, Hull to the Chicago Livestock show. We staid one night. On the way back he drove too fast and shakily. Off the road we went into the snow. It happened to be a nearly level place. A farmer pulled us back into the road. I drove his old Oakland the rest of the way.

I finished the lab work about the middle of December or a bit earlier. Prof. Whitson gave me other work, which only I could do. But I explained that I had the remaining time due me as leave. If I worked straight through he agreed to pay me through January. So I got the money back he took out of my pay the year before!

We packed up the non-furniture items, sold some of my old books and made ready to leave. We sublet the Madison apartment furnished. I left my wife, again enceinte, on December 31 and arrived in Fargo, January 1, 1930.

1930. Shortly after I started, the Dean asked me to take over an advanced course in soil management that also ran two terms!

I lived with the Bentons a few days and my sweet wife and Robert joined me about the middle of January. We soon got used to the cold. Our daughter, Mary Alice, was born February 28, - so then we were four.

The students were just fine. And I had time to write a bulletin called Preliminary study of the profiles of the principal soil types of Wisconsin, but without much time to

waste. At the request of several students and the Dean's urging I organized an advanced course in soil genesis and classification open also to sophomore students with high grades. So the two courses talked about in Chicago became five. Several students wanted to major in soils, but I explained that my continued employment was uncertain. Then one day the Dean told me that a petition urging a permanent appointment for me and signed by all the students I had had, was given to the President.

My friend Truog recommended a summer job for me, experting and planning a large estate owned by Mr. C. Wilbur Miller near Baltimore. The pay was to be $500 per month and all expenses from Fargo back to Fargo. For the lab work Truog offered me space at the University.

We left the Benton house with a few things stored. The Board had not acted on my appointment. We stopped in Madison and I took the manuscript for the bulletin into Prof. Whitson. The next day he showed that he was disappointed. Somehow

he had expected my work to discredit Dr. Marbut. He absolutely insisted on some bad changes if I were to get his approval for publication. I felt I had to do it. So the manuscript was wrapped up, ready to take to the printer in the morning. That evening I told Prof. Truog about it. He said, "you have a key, go in and unwrap it, put the text back as it should be, and wrap it up as before. Prof. Whitson will never know the difference." I did as he suggested and everything worked out as he said.

Then I went with Lucille as far as Chicago. She went to Grand Rapids and I to Baltimore.

The farm was some 3,000 acres of both good and poor soil for crops in the Worthington Valley north of Baltimore. Miller was a wealthy, driving business man and then President of the Davidson Chemical Company. I made a detailed soil map; collected many samples which I sent back to Madison; recorded the cropping history of each field as far back as I could get information; and studied the current records in detail. That year was the warmest and driest on record.

Before I left Baltimore, I had corrected and returned the proof on my bulletin.

Mr. Miller furnished me a car so I saw a bit of the country. But best of all I stopped a few times in the Peabody Book Store on Charles Street, and the old man persuaded me to buy a copy of Ulysses by James Joyce.

I returned to Madison very early in August by way of Ionia. I saw all the folks briefly and had a good touch of asthma, which left me as I crossed Lake Michigan. I hired a graduate student to help with the samples, wrote a report, and fixed up some good maps showing soil, sample locations, and fields. The report laid out a detailed plan and schedule for liming and fertilizer use for the next several years. Mr. Miller was pleased. Years later I talked to his manager and the plan had been put into effect.

The bulletin on Wisconsin soils was printed and I forwarded 100 copies in special covers to fulfill my obligation to Michigan State. I had a few copies bound.

In the meantime the Board at North Dakota had approved

my appointment and Mrs. Benton found us a furnished house and we stored a few things we had left in Madison.

I returned home to Ionia and went to Lansing. Dr. McCool was in trouble with the Dean over his expense accounts. A little later he was forced to leave. My hunch to get my degree when I did paid off.

I bargained for a 1929 Pontiac in good condition. We drove it back to Fargo with everything paid and a few dollars besides.

The Autumn term I organized an advanced course in soil chemistry and physics, one in plant nutrition, and an elementary course in geology. By now I had several major students. We liked the students very much, but our house was far from the campus.

1931. In the winter term I had beginning soils, an advanced course in soil management, and a seminar for junior and senior majors. How I had to figure to have enough good equipment and books for my students! But I fixed up a nice

study room for them and got permission to give the top students keys for it - my "honors" program. Any night until 10 p.m. the lights were on. They studied there together.

Also I talked to farm groups and wrote a bit for the farm papers.

I had an interesting discovery about magnesium deficiency on the experimental plots causing a phosphorous deficiency in barley. This was published in a note for the Journal of the American Society of Agronomy.

The Board of Supervisors in McKenzie County asked the College if we could make a soil survey and classify the land for tax assessment. Dean Walster and Director Trowbridge asked me about the idea. This had never been done before yet I said, "Of course we can do it". The County was willing to put enough money for the state share of a cooperative soil survey with the Bureau of Chemistry and Soils. The only bad feature was the split administration of the College. The Director of the Experiment Station, the Director of Extension, and the Dean of Agriculture reported separately to the President.

This meant working with Trowbridge who was a poor scientist and a worse administrator. Yet all the arrangements were made and Mr. Max Edwards was assigned as party chief. Unhappily, Chapman of our place was also assigned to the party and all that he did had to be redone.

In March, Dr. Henry G. Knight visited the College for 2 or 3 days. We had many fine talks together.

About June 15th we were glad to give up our house on the south side of town. The people who owned it, left town owing many people. The rent had been garnisheed but I was lucky to have a good lawyer to handle the matter. We made arrangements for another house in September near the campus.

Lucille packed the children in the car and went to Michigan. For about $277.00 per month I went out to Watford City and started the soil survey, and land classification of McKenzie County.

Besides Edwards and me, Goodman came a few weeks later, and Chapman was worse than useless. Roy Erickson came also in late summer.

These were new soils to me. I had only read about them. It was a big job for me to work up the descriptive legend and test it. M. B. Johnson, studying ranch organization was a big help. I met with the County Commissioners and explained the work. Once I took them out for a short field trip to look at soil profiles. As I recall one or two Bureau men came before the season was over. By fall it seemed that we had made but little progress yet actually we had. Also, on the side, I started a study of the partially truncated solodized-Solonetz soils. Dr. Walster had a field trip with us.

Before I left Fargo, I had a firm understanding about salary, which Trowbridge reduced while I was on the job! I wrote some strong letters to him and to Dean Walster and the matter was corrected.

I was back to my teaching about September 15th. We settled in the Weeks house near the campus and a little later had a student girl to help. We brought up our stored furniture from Madison and bought a bit besides.

In November I got a bit of expense money from the college, $30.00 from the Association of University Professors, and with a bit of my own went to the Soil Survey Association and Agronomy Society meetings in Chicago. I gave a short lecture on the relation of magnesium deficiency to phosphorous deficiency. (Years later (1947) I found a similar unsuspected relationship of great consequence in African oil palm in the Congo.)

After the soil meetings I went up to Ionia for a few days and then back to Chicago for the meeting of the Association of University Professors. By chance, I had lunch with an old professor of psychology of the University of Chicago (Will James), who told me how to get my students to read. (See Reading for soil scientists, together with a library (Revised, September 1962).

During this winter I spent much thought on how to get the soil survey and land classification going strong next year, including ways to make adjustments for distance from markets, grazing use, and so on.

1932. . Teaching went well and I began to have an increased number of real able students. It was difficult to get them excused from the poor agricultural courses to take the basic math and science. But the Dean and I managed. ← insert 45a.

In the Spring I got the College to hire J. K. Ableiter to help me with the land classification. Several students were prepared to work on the job and the Bureau sent several men.

We had many students around the house, including English majors, and we chaperoned many parties.

Again in the middle of June, Lucille and the children went to Michigan and I to McKenzie County. Gradually the party increased in size. Edwards and I had a big job getting all these men going. On Sundays I worked on the Solonetz soils and made many cross sections and examined many more profiles. Through Mr. Rice, the inspector from the Bureau, Dr. Byers agreed to do some chemical work on samples I collected.

During the winter I made some sodium determinations on both samples of the soils and samples of similar kinds of grasses growing on Solonetz and on Chestnut soils. Those growing on the Solonetz had a much higher content of sodium. From these data and the morphology it became clear to me that the explanation by Gedroi~~x~~z should be supplemented by the importance of the cycling of the sodium from the plants into the soil and back again.

[illegible] point I made some such observations on [illegible] of the soils and samples of similar nature [illegible] growing on solonetz and on Chestnut soils. Those growing on the solonetz had a much higher content of sodium. From such data and the morphology it became clear to me that the explanation [illegible] should be supplemented by the information [illegible] the soil is from the plants into the soil [illegible].

Dr. H. C. Hanson from the College and Mr. M. B. Johnson both helped me get the normal carrying capacity of the different kinds of soil nailed down. We collected data on marketing costs, and on the distance animals could graze well from water sources. A scheme was worked out to handle the wooded areas along the Missouri River and many other details.

In July we had a visit from Dr. Marbut, Mr. Rice, and Dr. Walster. At first Dr. Marbut objected to the length of the legend. insert 46a, 46b, 46c ~~But after we had had a good go in the field he seemed quite satisfied~~. He had not realized how detailed - very detailed indeed for those days - a soil survey had to be for accurate land appraisal.

By the end of the season the soil survey party got to be most too large. But it could not be entirely finished.

Again by September 15 we were all established for the school year. The economic depression plus the drought in North Dakota made money very short. Many banks had failed. College salaries were in for sharp reductions. Many began to look for other jobs, but I decided not to except ~~within~~ anything outside my own field.

But we all went out in the field together to look at the soils. Early in the forenoon Marbut called a stop near one of the high conical scoria hills. I climbed up to the top with him where we could see over a vast area. Off to our left was a sharp line in the grass with brownish green on the one side and yellow on the other. He said to me, "Kellogg, what soil difference makes that line down there?" "Dr. Marbut", I replied, "That line si not due to a soil difference. It marks an old, old field boundary. On one side the gramma grass is the original vegetation. In the old plowed field the needle grasses have invaded and these are now yellow." "No, no", Marbut said, "that's a soil difference." "Would you like to make a small bet", I asked him. But he wouldn't bet. "Well, anyway", I said, "let's go down and look at it." When we got down there Marbut just took one look at the profile with the spade. "I am amazed, I would have never thought that that had been plowed." I told him that I was glad that we had

had this little argument and explained that we simply couldn't map soils from the hill tops in this county. And if you got close enough to identify the grasses you might better look at the soils themselves anyway. He agreed.

And this is the way we went all day. Once you showed him the facts you were all right. But we had a final go in the late afternoon. We had driven south from the gently undulating till plain near Alexander and then into the edge of the Badlands. All of a sudden he said, "Stop, stop." As we got out of the car, still looking south, Marbut said, "Now here is where you can generalize. We have been going through this fairly steep grazing land; we have before us these gently rolling hills; and beyond is a nearly flat valley; you have three landscapes in all." I asked him, "Would you think that this is good grazing land that we have been coming through?" "Why yes," he replied. So I said to him, "Would you mind turning around and looking north?" His face dropped; every south-facing slope was completely bare. I said, "You see, Dr. Marbut this soil survey is being used to

determine the fair taxes owners must pay. We must see both sides of the hills. And now I want to take you down in that flat."

We drove on, and in the flat I showd him good Chestnut soils, solodized-Solonetz, and spots of Solonchak.

After dinner that evening I said, "Now, Dr. Marbut, that you have seen the soils and you know what is demanded of the survey, which kinds of soil in the legend would you combine?" "It's all right," he said, "all right."

I had to tell the owner of our house that we could not pay so much rent as we had, not because the house was not worth it but because we simply would not have the money. I told her what I could pay and she accepted. We also had 4 girls, three of whom paid for room and board.

Schedules were marked out for starting the land classifacation from the soil maps. Ableiter spent full time on this work.

1933. I worked up drafts of two papers that were published in 1934: The place of soil in the biological complex and Morphology and genesis of the Solonetz soils of western North Dakota. One went forward for early publication on our scheme for land classification. So with the students I had a busy winter.

Banks closed in Michigan and Roosevelt had to close them when he went in as President in March. But he brought a new spirit to everyone.

In May my salary for the year was only $1,920! Still

we were better off than many. Again in June, Lucille and the children went to Michigan and I to western North Dakota. We had some work left in McKenzie, which Edward had well in hand, and considerable checking. Goodman became party leader in Morton County and we started the work in Billings County under Mr. Edwards. I divided my time among the three counties, with most of it in Morton County. What a lot to do!

In July Edwards, Rice and I scouted Billings County and set up the legend for that survey. About 2 o'clock Edwards started back to Watford City.

Mr. Rice and I had finished our work in Billings County and were sitting in his hotel room killing a small bottle of local whiskey. It was very hot. Tommy Rice was going to Wyoming and I was going back to Mandan; and we had about two hours to wait for the trains.

He said to me, "Kellogg, whom do you think is going to take Dr. Marbut's place? You know he must retire quite soon."

"Well," I said, "I had always supposed that it would be Dr. Baldwin."

Tommy replied, "Well, if one of my fellow inspectors is made chief, I will be loyal to him. But I understand that Dr. Knight has said that it must be somebody from the outside."

So I mused, "What about Shaw? What about Conrey?"

I mentioned two or three others, ~~and~~ but each time Tommy said, "No, no, no! Then he mentions: Dr. Knight has said that the man who comes in must be a young man. He must have the scientific background to continue Dr. Marbut's great work; he must have a Ph.D., and he must have done something of great importance for the practical application of the Soil Survey."

I thought a minute and said, "I am afraid that doesn't leave Dr. Knight many degrees of freedom."

"Damn it, man!" Rice replied, "We want you to take it."

"No," I said, "Who ever heard of me, working up here in a little college that no one ever heard of? I have often thought that if they gave an examination for the job I should like to take it."

Mr. Rice said, "A lot more people have heard of you than you think. Why do you think Dr. Knight visited you last year?" And why Morton last summer?

"I haven't the slightest idea. I wondered a bit. You know, he hung around there for two or three days and spent most of his time talking to me," I replied.

Well, then Mr. Rice went on to assure me that this was definitely in Dr. Knight's mind. Yet I couldn't believe it. Our trains came and I went back to Mandan.

Since I got the maximum possible salary, $1,680, I would get absolutely no pay for working 12 months. So I decided to have a month's holiday - my first.

I left the work going well in Morton County, except for Layton, who was undependable. Roy Simonson had won a one-hundred dollar prize the year before, so he came along.

Sunday, August 13 we arrived in Fargo. We went by train with a brief stop in Madison to see Prof. Truog and the others. With one new paper just published and two in press, I felt good. In Chicago we met Lucille and took a look at the World's Fair and went on by bus to Galesburg where Lucille had left the car. We went on to Ionia for a while and returned to Fargo by way of the Upper Peninsula.

Morale was pretty low among the faculty when the autumn term began. We organized a teacher's union - an AFL union. I was elected to represent our local in the Trades and Labor Assembly for Fargo and Moorehead. I recall that we held the meetings in a sort of loft over a store.

C. C. Nikiforoff stopped in Fargo and talked to me along the same lines as Mr. Rice, and I still didn't have any stock in it.

In November I scraped up a little money to go to the meetings of the Soil Survey Association and the American Agronomy Society in Chicago. I also got $30 from the local chapter of the Association of University Professors to attend their meeting which came at about the same time in Chicago.

I met some of my students in Madison and helped them the day before. We stopped a day in Madison.

At the meetings I saw Dr. Marbut and other ~~big shots~~ leaders in the Department of Agriculture, and nobody said a thing to me about coming to Washington. Tommy reassured me that everything was going along, but I was convinced that he was having hallucinations.

The CWA came to Fargo in the late autumn. ~~The boys~~ Men even painted my office, but they were complaining about the low wages. President Roosevelt had arranged that common labor would get 50 cents an hour, semi-skilled, 80 cents an hour, and skilled $1.25 an hour. The projects were sponsored by local units of government, the county, the city, and the college qualified, ~~and~~ the local government furnished the materials. The labor was paid for by the federal government and the names were selected from appropriate regist~~rations~~ ers of people needing work. The maximum amount of work was 30 hours a week. I told the people ~~that~~ who came to me about their trouble ~~that they would~~ should first need to join their union to get any protection.

This matter came up at the Trades and Labor Assembly. All of the older men were opposed to th~~is~~e kind of schedule the President had laid down. The argume~~nt~~d ~~was~~ that a man was either skilled or not skilled. I ~~entered the argument and~~ insisted that the President had already set these scales and we could not negotiate them. If they insisted on $1.25 for everybody, they would all end up getting 50 cents an hour, which is just what the Chamber of Commerce people wanted. We argued for nearly an hour. Then I had the greatest compliment ever paid to me. An old Swedish carpenter got up and said, "Gentlemen, we don't know nothin' about the inside economical part of this; ~~Mr.~~ but Kellogg does and he's honest;

so I move we let him have his way." The motion passed in this form. I asked for two members to be appointed to work with me, and this was done.

Either that evening or the next morning, I called the crusty old judge in Bismarck, whose name I have forgotten, that I wanted to come out and see him. I told him why.

He said, "You don't need to come out here. I'm going to be in Fargo in a little bit and I'll talk with you there."

The judge's tone wasn't very reassuring.

Two or three nights later, just after I had gone to bed, the president of the Trades and Labor Assembly called me and said the judge was down at the Gardner Hotel ready to talk, but he wouldn't let any of the boys talk with him until I came down. So I got up and dressed and went down to the hotel, in the air ~~a minus 30 degree~~ temperature. was 30 degrees below zero.

The old judge said, "Let's go on up to the room."

When we were seated, he turned to me and said, "Young man, you can't get $1.25 an hour for all these men."

I said to him, "Judge, I never had any such intention. All I want is for the city and others sponsoring these projects to adhere to the President's regulations. For example, the city set up a CWA project to clean up the water front and asked

for some 200 common laborers. Of course, our boys took the jobs--they didn't have anything to eat. But then the boss went through and picked out 10 men who were skilled blacksmiths and sent them up to the city shops to sharpen tools. Those ten men should have had $1.25 an hour, according to the regulations."

The judge admitted, "You're right on that. But it doesn't take ~~a group of~~ highly skilled men to paint a bridge."

I asked him, "Judge, how do you go about painting a bridge?"

He replied, "Well, I don't know. How do you do it?"

"Well," I said, "I haven't the faintest idea. I have noticed that sometimes they start out with a coat of red paint and then go to black or gray. My whole point is, Judge, that the federal government is paying these wages and the local governments are paying for the materials. The public has a right to expect good work. Maybe you need only one man on this bridge project who knows how to paint bridges, but certainly you need that one. I suspect that you may need more--perhaps one skilled man, some semi-skilled men, and three or four common laborers for a crew."

By this time, the old judge had warmed up considerably.

I told him, "We have now hit on the heart of our problem. Our people are desperate. Fifty cents an hour for 30 hours a week is only $15.00 to raise a family in Fargo, but they will take ~~it~~ that if there is no better opportunity, no matter how skilled they are. But the President wanted to avoid that kind of thing."

The judge said, "What would you do if you were me?"

I answered, "I'll reply to you as honestly as I know how. If I were in your place, I ~~w~~ would have a 3-man committee, one appointed by the Trades and Labor Assembly, one appointed by the mayor and his crowd, and one appointed by you. This committee would go over each job and determine a reasonable proportion of skilled, semi-skilled, and unskilled people to carry on the work efficiently and fairly according to the regulations. If you do this, for our part we'll agree to check over the rolls and see that the men who are claiming to be skilled are in fact skilled. I found the other day by accident that one of the so-called skilled painters had been braking for the Northern Pacific Railroad for the past 20 years. We'll do our best to clean up these rolls."

He answered, "Young man, I'll buy that. Whom do you think I should appoint for my man?"

"Well," I replied, "I should take one of two men. Father Ryan of the Catholic Church here is one of the most sincere men I know. He knows more about the social conditions in this town than anybody else--far more than the mayor. Dean Dolve of our School of Engineering is an honest, competent engineer who knows practically nothing about the social conditions in this town."

He said, "I'll take the Dean."

So everything was fixed up and we had no more trouble on this score.

One day while I was in my laboratory talking with Dean Wallster, a big man came in and told me that he was arranging the program for a meeting of the Master Painters for a wide area, centered around Fargo. This included North Dakota, part of South Dakota, and part of Minnesota.

He said, "I'm looking for a speaker on labor relations, and I would like to have you accept."

"Well," I told him, "I don't know anything about the craft."

He said, "You don't need to. We want somebody that knows the economical part of this. We don't want one of those rousing labor leaders but a good conservative speech."

(At this, Dr. Wallster put his hand over his mouth and rushed out of the laboratory.)

I went down and gave them their speech about what I thought the President had in mind. I made much of the fact that now young men were graduating from high school and had no jobs. "All of us have the habit of working and we'll continue to work; but what a pity it would be for these boys to get the habit of not working." I really think I sold them the CWA regulations.

Imagine my chagrin the next day in the FARGO FORUM to see myself quoted as saying that the way to solve the problem

was to cut the length of the work week. Of course, I had said no such thing. Naturally, I was a bit fearful about going to the next meeting of the Trades and Labor Assembly. But I discovered that not a single man in that room believes that I had said anything of the kind. So there were no fireworks.

Just before Christmas 1933, I received a peculiar letter from Dr. A. G. McCall of the Bureau of Chemistry and Soils asking me to accept a temporary P-3 ($3,200 minus 15 percent) appointment to work with TVA. I had written a refusal but before I sent it, a ~~letter~~ personal telegram came from Tommy Rice, saying this was the beginning of my appointment to be head of the Soil Survey and also a more formal letter. I told Dean Walster about it and suggested leave from February 1 to July 1. I told him I would take whatever time might be necessary to help Ableiter finish up the land classification if I was raised to a full professor. After a while President Sheppard came over to talk about it. He agreed.

Some sort of skulduggery began with Director Trowbridge, now on his way out, to get Dr. Alva Benton connected with our land classification. Ableiter and I agreed to show him the work but <u>not</u> to explain it. This effort came to an end.

1934. January was very busy indeed, making ready to turn everything over to Ableiter. I agreed to grade the papers of the seniors in Washington.

In January I continued to attend the Trades and Labor Assembly. It was time for election of officers and I was immediately nominated for president. I explained that I would be leaving at the end of the month and made my first political speech for my new steamfitter friend. He won. (Afterward he made a bad mistake. A teamsters local was organized and he didn't check their credentials carefully. They had some Commies in the group and called a strike in severe cold weather, and even refused to deliver coal to the hospitals and schools. Of course, the people wouldn't stand for this and broke the strike. This was a great pity.)

Had I stayed in Fargo, I would have been sorely tempted to run for mayor on just one platform, which I would have repeated on every street corner: "$15.00 a month to raise a family in Fargo." This was considered fully adequate by the mayor and his friends in the Chamber of Commerce. Even further, they were trying to get regulations to deny any sort of relief to people with a car or with a home. Many of the men now out of work were good, honest workmen who had followed the copybook maxims and had fallen into the trap of buying homes and cars on time. To have sold either in the depths of the depression would have made them lose what savings they had. I probably wouldn't have won, but I would have had a lot of fun.

This concluded my one and only participation in the American labor movement!

Since I had been granted leave, and matters were so uncertain, I left Lucille, Robert, and Mary Alice in Fargo and arrived in Washington, D. C. February 1. Tommy Rice met me and had a hotel room for me. I was sworn in and brought to Dr. Marbut. The TVA cooperation was out for the present. I got a room on M Street and did more or less odd jobs.

I spent Saturday afternoons in the Congressional Library and checked out dozens of books I had been unable to get in Fargo.

I had brought some of the McKenzie County maps with me and finally showed these to Dr. L. C. Gray, who headed a submarginal land purchase program. He agreed to a project after I explained how much the county commissioners wanted it and we had the land classification done.

About March I told Tommy Rice, "I've been here a month and no one but you has said anything to me about taking Dr. Marbut's place. Soon I'm going back to Fargo where I have important work to do." In a few days I was called into Dr. Knight's

office and the matter was explained.

Since the director of the Washington State Experiment Station asked for some one to come and help untangle the soil survey program being promoted by the new Planning Board, I was directed to go. I also agreed to meet with the Commissioners of McKenzie County.

I stopped in Fargo on the way out and talked the whole matter over with the people at the College. I wanted M. B. Johnson to be project leader. I made arrangements by phone with M. B. to meet him in Williston on May 5.

Then I went to Pullman, Washington and toured the areas most needing soil surveys with my old friend, L. C. Wheeting. What a mess! But with this background I got it straightened out in the autumn. On May 6 I met the Board in McKenzie County. They were delighted with the proposal.

(The Federal Government acquired the wild land or control of it and formed a grazing association over the next few years. This relieved the county of roads and schools in those areas

and later gave them $75,000 per year for roads and schools elsewhere. I am very proud of this McKenzie County survey. It has paid for itself every year since it was finished in 1934.)

I stopped for a few days in Fargo, checking over the land classification and pushing M. B. Johnson's appointment. I returned to Washington about May 16.

The Congress had cut the soil survey appropriation so I scrambled for money. We got some to go ahead with the Puerto Rico survey.

[By now H. H. Bennett was trying to get the staff and make much trouble for me personally, which never let up so long as he lived. Marbut had never trusted him, nor had Knight. He associated me with them and transferred all his hatred of them to me.]

~~Also I was offered a job with~~ The Forest Service asked me to work with them on the shelter belt program. First the Lettermen ~~(sent~~ to North Dakota). We arranged for Frank Hayes to handle ~~the~~ the soil phase of the program and he did a fine job.

Insert 64a, 64b, 64c, 64d, 64e, 64f, 64g, 64i

The soils work in the Department was confused and uncoordinated and remained so for many, many years. No strong leader replaced Professor Whitney. The bureaus of Chemistry and of Soils had been combined in 1927 as the Bureau of Chemistry and Soils. Dr. Henry G. Knight a chemist, was selected to be its Chief but before he came A. F. Woods, newly appointed Director of Scientific Work, brought in his old friend Dr. A. G. McCall who became the officer in charge of the soil research. I reported to him when I first came into the Bureau. Although Bennett had left for Interior when I arrived, for several years Dr. McCall had 5 principal divisions: Soil Survey under C. F. Marbut, Soil Chemistry and Physics under H. G. Byers, Soil Microbiology under Charles Thom, Soil Fertility under Oswald Schreiner, and Soil Erosion Investigation under H. H. Bennett. Each of these men had a stronger personality and, except for Bennett, a wider scientific reputation than McCall.

Mr. McCall was simply a man in a job. He had limited intellectual ability. Many thought him dishonest in his dealings but I am completely persuaded that they were wrong. He made a lot of mistakes and made bad moves, but from stupidity, not malice. For example, I feel sure that he had promised Dr. Conrey of Ohio Dr. Marbut's place without consulting with Dr. Knight. Then he made it up to him by a cooperative agreement that was not in accord with policy under which Conrey got better-than-usual travel allowances and double salary (with Ohio State) for a while. This got him and me both in trouble. Besides that, Dr. McCall had become very lazy by the time I knew him.

Had McCall been a strong scientist with imagination and a vigorous administrator, the soils work would have been so integrated and such cooperation would have developed with those agencies dealing with crops, trees, range, economics, and engineering that the various splits would not have happened. Oh, what a great opportunity he missed to the

detriment of the Department, of agriculture, and of soil science!

Bennett was a very ambitious man, especially for salary and rank. He too had severely limited scientific ability. But he made up for it with a burning ambition and by bombastic oratory and writing.

From time to time over the years, members of the Soil Survey staff were asked to make special studies abroad. Bennett wanted very much to go on these but Marbut did not trust him scientifically. (He and Rice had made some serious mistakes in their Alaska soil survey during the First World War). At Bennett's request, Rice and Hearn had interceded with Marbut to let Bennett go. Against his own judgment Marbut agreed and so he went to Panama and later to Cuba, with Allison. (Soils of Cuba, Washington, 1923).

I got involved with this book in a curious way. After it was published, a world sugar congress was held in Cuba. Some of the Dutch stationed in Indonesia looked at the soils and the survey in the field and wrote a blistering and unfair

attack in Dutch, which was sent to Bennett. He had no idea how to answer it and sent it to Dr. Marbut in the winter of 1933-34. Marbut wrote a blistering reply and asked me to "fix it up" in February 1934. It took a lot of editing to make it publishable in any language. The edited reply was translated into Spanish and published in Cuba.

Although in later years Bennett claimed an early interest in soil erosion, this is completely false. (The exhaustive bibliography of American soil science, prepared by the USDA Library for the 1927 Congress of the International Society of Soil Science lists some 70 papers on soil erosion but not one by Bennett.) In fact, while an inspector in the Soil Survey he discouraged field scientists from considering erosion and even cut out phases of eroded soils they had in their legends!

About 1925 he visited a soil survey party headed by E. W. Knobel in the "clay-pan" area of eastern Kansas or Oklahoma where erosion was destructive by exposing lower

horizons of soil that were extremely unproductive with the current methods. Knobel showed Bennett his cross sections and descriptions and suggested he "write it up". Bennett told Knobel that he should write about his own work. But Knobel persuaded Bennett that he could do it better. And he wrote it. For years afterward, Knobel got a lot of joshing about being the secret father of the Soil Erosion Service.

Previously Bennett had written much for outside pay as well as his soil survey reports and the like, but he had had little to say about erosion despite the growing literature from research and the fine literature of the Bureau of Soils in its earlier days.

His first articles started to appear about 1926. People began to be interested and he was off! Each new one became more extreme than the previous one. These worried Dr. Marbut and Dr. Knight a great deal, but they did not do much about it. To get some flavor of the drastic change in Bennett's thought, one can read the glowing account in his book, The soils and agriculture of the southern states (Macmillan. New York, 1921)

of the Black Belt. He extolled its great fertility and compared it favorably with the flood plain of the Nile! This may be compared with his extreme descriptions of desolation on the same area from soil erosion in his Soil conservation (McGraw-Hill, New York 1939). The soils may have changed a bit but not a fraction so much as the description, neither of which is approximately accurate.

Then around 1929 the Department received funds for research in this area of soil erosion. Although the earlier work in Texas and Missouri was influential there can be no doubt that Bennett's articles and talks sold the Congress.

When this appropriation was received about every mistake that could have been made was made. First of all, the funds were split among the Forest Service, The Bureau of Agricultural Engineering and the Bureau of Chemistry and Soils. The Forest Service pretty much went off by itself with a program for the kinds of areas within the National Forest.

Bennett was put in charge of a new division under McCall. He was given a small raise but not given the full rank of a division chief (P-6). This rankled him terribly and he hated both Knight and Marbut from then on. Throughout much of his later writing and in books by others written partly or mainly from his dictation, such as Lord's Behold our land (1938) and Brinks Big Hugh (1951), cut out of whole cloth vicious cracks are made about Marbut, Whitney, and others. Concocted stories are even told that Bennett was sent out of the country to keep him quiet!

The action regarding Bennett's rank was wrong. He was either qualified to head the new division or not. Either someone else more nearly qualified should have been made chief or Bennett should have been given the full rank.

Yet even worse, perhaps, was the organization of the staff at the new erosion experiment stations. The local director was a soil scientist (or technician) reporting to Bennett and the assistant director an agricultural engineer! I was

told that the Secretary's office was continually heckled by disputes between the two bureaus. This experience soured Bennett on cooperation. He determined that once he got things under his control he would "call the shots" himself and have no cooperative agreements. This resulted in the complete failure of TVA to work out some arrangement with him for erosion research in the Valley. TVA was committed to cooperation with the land-grant colleges and Bennett would not consider any research not completely under his control. As time went on Bennett became more sure of his position and he relaxed this extreme position a little.

Bennett had the prestige and money to become the national leader in all soils work. But he ~~became~~ was so antiscientific that he alienated most of the soil scientists in the United States. They would not work for him and could not even appear to agree with him. Yet he did get enormous public attention to soils and soil problems. For this, and for

insisting on a combined approach of soil science, plant science, and engineering based on some kind of soil map, he deserves great credit.

Over the years the bad influence of Bennett and the weakness of A. G. McCall were felt. They both terribly downgraded soil science. Even in the fifties engineers and plant physiologists were being appointed to key soil-science positions in the Department with few people noting the utter absurdity of it.

Even in the 1930's I wondered many times about the potentially important program that Bennett got started for about 95 percent wrong reasons. Certainly people would soon learn the facts about the soil erosion he had so enormously exaggerated and then what? Would it be possible to save anything for the real opportunities?

June . Earlier I had wired Lucille to sell the furniture, ship the books that I had packed, and come on. But by then Robert had scarlet fever and they were quarantined during one of the worst periods of dust blowing Fargo had had. When she could, she sold the extra things and drove to Michigan.

In the meantime Milton Eisenhower insisted I write a bulletin on soil blowing. He wanted it in a great hurry. I got this done in May or June but printing was slow. (Soil blowing and dust storms. USDA Misc. Publication 221.) (It came out the very day the big storms began in the drought area of the southern Great Plains in 1935.)

Lucille came ~~to stay in June~~ in mid-June for a month. I had found a little apartment, a hot one as it turned out. And about now we were installed in new offices in the South Building.

On July 1, 1934, I became Acting Chief, Division of Soil Survey, and Dr. Marbut's time was extended for a year "to complete the soil series descriptions" and to act as my advisor, which he did on soil science but would not on administration. I remember the only time I had asked about

1934. Milton Eisenhower was then Director of Information. He was quick, affable, and very ambitious. He had been a political appointee of the previous Hoover administration. Before they left, however they managed somehow to get Milton a high-ranking Civil Service job. The Democrats had been warned about him. But Milton was a member of the Press Club and invited the boys down many times for cocktails and lunch. Soon he had their confidence, although basically he could not have cared less about the New Deal. He told me so one evening at his home. He had literally cocktailed himself into the New Deal. Also he had a gracious wife with money and social position and a brother of importance in the Army. He told me many times of helping him write his reports long before 1938.

whom to raise. "Young man," he said, "do not think you can make soil scientists out of those men. They will never be more than cogs in a machine to do as they are told. Your job is to tell them." "Dr. Marbut," I replied, "every man that works for me will have at least one chance to prove that he can do something better." — insert 66a to 66e

During this period I also wrote two other long bulletins, one on land classification, to which I added Ableiter's name (USDA Tech. Bul. 469, 1935), and one on The development and significance of the great soil groups of the United States (USDA Misc. Pub. 229, 1935). Both were reprinted. The second one was, in fact, widely used as a college text. Then I wrote several shorter pieces.

We went into the new fiscal year on July 1 with a shortage. I was supposed to drop people but I would not since there was no relationship with their real efficiency and the ratings given them by Dr. Marbut, and mainly by his administrative assistant. But before the year was one-half gone, we had money enough from several sources to cover the

After I was nicely settled as (Acting) Chief of the Soil Survey, it became obvious to me that Rice and Nikiforoff had been assuming that I would be de juro Chief but that they would run the Soil Survey. It also became clear that Mark Baldwin had the same idea and didn't like it. I really had a job to maintain loyalty and order in the staff because most of the men in the Soil Survey were older than me and the inspectors very much older.

Baldwin was not only unhappy on this account but also because he had fully expected to take Marbut's place. In this expectation I think he was honestly convinced from something that Marbut had said and that he had misinterpreted. I was told much later that Earlham College was pressured to give him a D. Sc. because if they did he would have the position. Dr. Knight had said that the man would have to have a doctorate degree, but, of course, he meant a Ph. D.

Baldwin was the best student among the inspectors, but slow. His maternally inclined wife almost babied him. It

was a shame that they could have no children. Also nearly everyone in the Soil Survey adjusted their work and schedules to Baldwin's wishes. Basically he was a very ambitious man but both timid and jealous. He was extraordinarily critical of the writing of other soil scientists so he supposed that others similarly criticized his writing, so he wrote very little and we had to press hard to get that. Yet he hoarded his ideas and notebooks.

During the early days of the war, I recall that the men preparing "going conditions" for tanks and the like wanted to borrow his notes from Mindanao, P.I., which I had tried to get him to prepare for publication for years. He delayed giving them the notes and came to me for advice. "If I give them my material it will be out of my control. I don't know what to do." I looked straight at him and said, "But Dr. Baldwin, our country is at war!" So he got up to walk out and said,"Yes, and I really should give them up".

Baldwin was quite difficult on some days and splendid

on others. He never accepted me as I should have liked but I feel that I got as much from him as anyone could have. He had no administrative acumen at all. In the forties he became mentally very ill, had a partial recovery, and then retired.

Dr. Baldwin passed on the rumors of the supposed influence of Nikiforoff and Rice to James Thorp and he was also very antagonistic toward Nikiforoff when he came back from China. I remember that sometime in 1935 or 1936 Nikiforoff insisted with great force that he must be made an inspector. I tried my best to explain to him that this wasn't his best opportunity, which it clearly wasn't, but he took my refusal with great resentment and became increasingly difficult to work with. It was a great pity because he had a lot of talent, most of which he used to down grade other people and ideas. Basically he was an aristocrat who had lost out in the Russian Revolution and felt a great injustice that he should have to take direction from basically peasant

people. Gradually over the years my patience wore thin but I was able to keep him from hurting the work very much.

I tried him on research without much success. He had no real concept of the scientific method. Whenever I gave him a problem he made up his mind on the answer and set out to prove it.

Much of what he wrote, I suspect was translation and rewording from the Russian papers that none of us at that time had read in the original. Most of his mapping was poor; he simply couldn't see the sense of detailed surveys and had no interest in agriculture. Except for Prosdoff, no one was able to work with him cooperatively except as a technical assistant.

Yet Nikiforoff was very gay and good fun most of the time, so long as we didn't talk about soils. Whenever I proposed some plan or idea to him I always got the same answer: "The basic approach is fundamentally wrong." He helped a bit with the first edition of the Soil Survey Manual

~~He helped a bit with the first edition of the Soil Survey Manual~~ and a little with the Yearbook but refused to do a thing with the second edition of the Manual after he came back from his assignment to the Office of Strategic Services after the War.

Mr. Rice had one of the best minds in the group. He could write well. But he was a lazy man. If I would step in his office he would always move forward at his desk. This was to close the drawer that contained the detective story he was reading. Yet in conferences he was good. And he had a remarkable memory for what he had seen and heard. On net, he was helpful so long as he lived.

After a few years this quarrelling between Rice and Nikiforoff on the one hand and Baldwin and Thorp on the other quieted down, especially after the top staff was strengthened with Ableiter, Moon, and Hayes.

Hearn was not a strong soil scientist but he did keep his reports and work more nearly up to date than any of the other inspectors.

roll and even to make several promotions.

In July and August I visited some of the surveys in the northern states.

Lucille and the children came in September and we took a furnished house on 26th Street South in Arlington and life became more nearly normal. Also my position was regraded and my salary became $4,600 so we made out better and paid up all our debts again.

In the middle of September we started one of the most important cooperative enterprises we had for several years. With Director Moores of the Tennessee Agricultural Experiment Station and J. C. McAmis and H. A. Morgan of TVA a plan was worked out to make detailed soil surveys in the Valley, with TVA putting up a substantial part of the funds.

This was a great experience. Moores and Marbut disliked one another cordially. I tried to reassure the old man without lying. I said, "Of course all one needs to do is to drive across Tennessee to see that the soils are related to

the geological pattern. I never once mentioned the word "profile", but talked a ~~lot~~ about the importance of fertilizers and soil surveys designed to give farmers good recommendations based on the work of the Experiment Station. It was plain to see that Director Moores was beginning to think that the U. S. Department of Agriculture was coming to its senses and put in charge of the soil survey a man that knew something about soils and agriculture.

I don't recall whether it was this time or later, while waiting a few moments for Director Moores, I looked over his old bulletin files. I saw one on The soils of Tennessee, published in 1897 and containing photographs of soil profiles collected by the Glinka method. When the Director came in, he exclaimed, "You wouldn't be interested in that old thing." I told him that it was terribly interesting and would like to borrow it. He said, "It's way out of date and it's the only copy I have." But after buttering up the old boy a bit he let me borrow it. Later when I showed it to Marbut

he was amazed. I had much of it copied and then sent it back to the Director telling him I would pay $25 for another copy if he could find one. He sent it back saying that if I wanted it so much I could have it. It is an American classic by Major Vanderford of the Confederate Army, who saw the Russian exhibit at the Chicago World's Fair in 1893. The famous Russian soil scientist, V. R. Williams, was the young attendant who explained the soil profiles to him.

I got along famously with J. C. McAmis from whom I learned a lot about agriculture and also how not to administer a program. Like many I about fell in love with Dr. H. A. Morgan. He was a great philosopher in agriculture and knew what he wanted, although he didn't quite know how to get it. During the next few years I wrote some things for him based on his explanations. Commonly I needed to try and try again until he said, "Now you've got it." He could see his ideal castle on the distant hill but had little concept of how to get through all of the brush and across the streams to get to it.

When the Act setting up the Tennessee Valley Authority was passed, which gave them the old World War One fertilizer plants at Muscle Shoals, nearly everyone assumed that the Authority would emphasize nitrogen fertilizer. Very wisely they emphasized phosphatic fertilizers instead. Millions of acres of soil were devoted to row crops that should be converted to grasses and mixtures of grasses and legumes for hay and pasture, either permanently or for a large part of the rotation cycle. The great limiting factors in doing this were phosphate deficiency, lime deficiency, and a lack of skill in animal husbandry among many farmers. To make the conversions in the South cheap electric power for refrigeration was also vital.

Dr. H. A. Morgan exhibited great foresight and enthusiasm for improving phosphatic fertilizers, for reducing their costs to farmers - especially by raising by 2 or 3 times their concentration over the ordinary, low-analysis superphosphate -, and by a program of test-demonstrations on operating farms that would adjust the other practices in order to get the full benefit of the interactions.

After the arrangements for soil surveys in the Tennessee Valley were all approved I wired J. W. Moon, then in Michigan to meet me in Knoxville. Thus we got this very important work started in good hands.

Our budget hearings went well on September 25.

A bit later I had a nice reunion with Ableiter and many of my former students in Belfield, North Dakota, for the inspection of the Billings County survey. The soils were terribly dry and no crops were harvested in the area west of Bismarck. ← minnesota and Dak

The Soil Science Society meetings were in Washington. Naturally I received a lot of advice on my new position. A very few were quite cool. Apparently several had expected "the call". (Austin Patrick spread a great scandal that my wife was a niece of a prominent Democratic Senator from North Dakota and that he got me the job! This had all the qualifications of good scandal - no truth at all, or even possible - no democratic senators ~~in~~ North Dakota, nor and no relations of my wife.

While the Soil Survey work was getting established in the Tennessee Valley I made frequent trips down there to work with Moon and the others. A serious complication resulted from the team of geographers who was making general-purpose maps in the Valley. The presence of this group had a lot to do with hurrying up McAmis and Moores on the Soil Survey. Some professors from Chicago had sold a Mr. Draper, who worked under A. C. Morgan, about the silliest scheme of rural mapping I ever saw. This team was headed up by a G. Donald Hudson from Chicago. He had some bright boys if they could have had some training in something. They set themselves up as experts in just about everything. They drew out areas on air photographs with extraordinarily complicated symbols consisting of long fractional codes. Each digit or letter in the numerator and in the denominator stood for some special class of crop, soil, slope, trees, and so on, and so on. Like the Soil Conservation surveys there was no classification, only note taking on the air photo.

We talked about cooperation, so I went out with them for a day or two. One of them would look at a stream and call it polluted or unpolluted with no test of any kind. They would look at the stubble of harvested field and indicate the yield in bushels. They would map vistas within the fraction. One of the boys pointed out to me a top-class field of alfalfa. The only trouble was it was lespedeza.

The monumental deficiencies that were perfectly apparent to any experienced field man were not evident to Hudson and Draper. So they continued to waste money on this stuff for four years. I recall that we had a summer meeting in the Valley. Wellington Jones of Chicago, ~~who~~ he did not support the scheme, asked me to take one of the sheets and draw a proper soil map over it. Of course there was no correspondence between the two.

Finally it became evident in TVA that no one could use these maps for anything. So in 1938 the Board asked me to make a study of them and a recommendation. And that was the end of the business.

And there were others, it was said that Tugwell, whose arrogance, in those days, I could not endure, met me on a train, and got me appointed. Later I checked out the whole business with Henry Donovan, who handled personnel matters for Dr. Knight at that time, and he assured me absolutely that only Knight and Marbut jointly made the decision.)

We had a special banquet to honor Dr. Marbut. Prof. Whitson had changed and asked to give the speech presenting him a gold watch. He did well and this pleased me no end.

Plans were made in November, or earlier, I don't remember, for a soil survey of Hawaii.

The bitterness between the Department of Agriculture and the Department of the Interior over Bennett's Soil Erosion Service worsened. Silcox, Warburton, and Knight got me to write material for Secretary Wallace to use in order to get him back into the Department of Agriculture. They all assumed that his outfit would go into Extension.

The manuscript for the land classification was ready in the late autumn. Dr. Knight and others told me that the Bureau of Agricultural Economics would object. I said, "Well, if there is something wrong about it, I should like to know". "That has nothing to do with it", Knight explained, "They will not let anyone else even use the word 'economics'". But it was sent over. All that came back was praise and Dr. Knight was astonished.

1935. From time to time for the next few years I visited the work in the Tennessee Valley. In January arrangements were made with USDA for me to help TVA defend itself in the Ashwander case. I worked much on this with Sessions and McAmis. And of course the continual arguments over organization of soils work.

Insert 72a

I also had a bad personnel case, inherited from long ago - Miles Beck. He had lied, over reported what he had done, etc. I ordered him to a party in North Carolina "on or before April 1" and later recommended his demotion.

During February I spoke at a Farmer's meeting in Ithaca. I had lunch with Prof. Lyon and dinner with Floyd Harper. I nearly froze at both places.

When I returned to the office I had a terrible cold coming. Lucille had brought out some clean clothing and my office had tickets from me to go to Birmingham and appear as an expert witness for TVA on the Ashwander case before the Federal District Court. I sent Smallwood out for a small bottle of whiskey and a lemon. I fear this embarrassed him but the need was great.

Shortly after getting on the train I had a small meal. I took whiskey and lemon and went to bed under extra blankets. I slept well and awoke next morning with no cold.

Fly, the attorney for TVA, did a good job. Old Judge Grubb and the opposing attorney let me put more in the record than I had hoped. We knew that the Judge had his mind all made up against TVA. The record was for the New Orleans Appeals Court. (This Court reversed the District Court and the Supreme Court upheld the Appeals Court.)

As I expected, before this was to go into effect, about August 1, he resigned in a great flourish. I had people primed to get it accepted immediately. So when his usual cancelling letter came the next day it was too late!

In March arrangements were made for Dr. Marbut and me to go to the 3rd Congress of the International Society of Soil Science in Oxford and Marbut would go on to China. I planned to go with him across Siberia and visit the soil survey in Hawaii.

Milton Eisenhower was Director of Information and had cocktailed his way into the New Deal, although he had little sympathy with it. At first I trusted him too much. He was able but a pure opportunist with no real convictions about anything. He pretended to help me and got me to do a great deal of work for him.

I recall a curious meeting in Warburton's office (he was Federal Extension Director) about March 22 after the first big storm in the so-called Dust Bowl. Bennett was there and great stacks of my bulletin on soil blowing were all around

the room; and Bennett had none. (Good reviews of it were carried in every Great Plains newspaper).

Bennett's outfit was transferred to the USDA and its name changed to Soil Conservation Service. The new organization for it, nursed by Eisenhower duplicated about every existing activity of the Department. But the agreement with the White House provided that it not be broken up.

On March 27, Secretary Wallace announced that the Soil Erosion Service would take over the erosion research of the Bureau of Chemistry and Soils. A few days later he wrote Bennett an extremely critical letter and told him that Milton Eisenhower would act for the Secretary. This committed Milton to the SCS. I worked very closely with Milton in those days and answered thousands of questions. Milton got to hate the assignment and far too soon told Wallace that Bennett was ready to go on his own. After that he defended them so long as he staid in USDA. (When he went to Kansas State as President after the Second World War he reversed himself completely.)

Then near the end of April Congress passed Public Law 46, which officially established the Soil Conservation Service with very wide powers. In May, the whole crowd of Bennett's group was "blanketed" into Civil Service, which shattered the USDA Personnel Officer, Mr. Stockburger. They had several good men, but more were unqualified by current standards.

A. G. McCall tried hard to get Knight to leave me as leader of the "scientific side" of the Soil Survey with himself as Chief, but Knight refused.

Dr. Knight was under terrific strain with his trouble with Bennett and keeping the Secretary from going overboard on "Chemurgy". While in Washington State he was hospitalized with coronary thrombosis. This meant that I could not go on to China with Dr. Marbut or even take the field trips in the United Kingdom after the third Congress of the 1935.

I had given Baldwin responsibility for soil correlation. But he was quite unhappy that he had not been made Chief

and Bennett's group fed him rumors that I wanted him out. Baldwin was very ambitious, but when he remembered that he was a Quaker, was very modest. I felt that he was basically frustrated because he had a maternally inclined wife, but no children.

Despite all this turmoil, I wrote papers for the Oxford Congress, for the Yearbook, and started a Soil Survey Manual. All the inspectors (soil correlators) opposed this. There never had been a Manual, and the methods were shrouded in mythology. One was supposed to travel with them and learn by osmosis. It was very difficult to get anything out of them. A manual was desperately needed, yet they would react to my drafts. Joe Moon and Frank Hayes were especially helpful.

We got a nice increase in funds for the Soil Survey and so for the moment money troubles were over. While out of town, the final papers came through for my position as "principal soil scientist" (P-6), the grade for a division chief in those days. So the "acting" came off my title.

Dr. Marbut and I left for Oxford on a slow boat out of Baltimore July 17. We had wonderful talks about soils on the way. Dr. Marbut then said that he had been wrong about pedecals and pedalfers, and that if he could do his classification again, he would group the Prairie soils with the Chernozems.

After arrival at Oxford on July 29, Dr. Lipman called a meeting of the American Delegation - Dr. A. G. McCall, Dr. C. F. Marbut, Dr. W. P. Kelley, Dr. C. F. Shaw, and myself.

It was good for me to meet so many of the "greats" or "near greats" like G. W. Robinson, Stremme, Milne, Reifenberg, Greene, and many others. I visited Rothamsted, Jealott's Hill, and a soil survey in the Vale of the White Horse.

Marbut and I went to London together August 9, directly after the Congress. For the last time I saw him when he started to Moscow for the trip to China, where James Thorp was working on loan from the Soil Survey. I recall a brief time in the bookstores - but long enough to get a few Joyce and Lawrence items. Walter Lowdermilk and I went back on the Baltimore Mail Line together. I was back in Washington about August 17. Lucille and the children were in Michigan.

I found much to do all the way around, in my office, with TVA, and on the manuscript for the Manual.

I took a few days leave and went to Michigan. My father was all enthused about the prospect for oil and talked me into buying the old Heath farm about one mile east of the home farm. It had some ~~60~~ 98 acres, about one-half fairly good, but "run down" soil, two old barns, and a good brick house. I had costs to clear up the oil rights, to get a loan from the bank, and to get a tenant. I arranged to put back into the place all excess over my costs. (Near the end I did not and kept a few dollars.) Later we fixed up the house and one barn, (which raised the taxes). We paid a bit on it; I borrowed some from McKericher and got $1,750 from the Ionia State Savings Bank. The farm cost $2,500 but I had legal expenses of various costs in addition.

This turned out to be an irritating 9-year headache. But I got back my expenses. The wind destroyed the poor barn and I had ~~$1,2000~~ 1000 insurance. I finally sold the farm

in 1944 for $3,000. Even with the cheating of tenants I came out a little over $600 ahead.

The family drove back with me about August 26.

About August 28 we got the terrible news that Dr. Marbut had died of pneumonia in Harbin. He had been taken off the train and put in a hospital. James Thorp arrived a day or so before. My hardest job was to write the obituary for Science.

I worked much with Milton Eisenhower to save the Soil Survey. But I did push him to get a job for A. G. McCall with Bennett. After all, the Secretary's aides had created the problem. But Knight also asked me to write a job description for him in my office. Already I had a senior position set up for soil survey interpretation in my office. But he could not do that.

About 79e

About October 13 I started a trip with Carter in Oklahoma and Texas. I had expected to find evidence to fire him. Miles Beck had liked him. I think Carter expected something like this because at every area, he "just happened"

In early October we were informed that Pony Lesh had got drunk and was found by police in a sugarcane field. We wired the boys to put him on a boat and send him back. Arrangements were made for Macy Lapham to meet him at the dock and put him on a train. Macy hired a big Swede to watch him while he slept before train time. He came back while I was in Texas. When I saw him I gave him a few hours to resign, which he did.

to have the correspondence. It was a wonderful trip. I soon realized that I was with one of the best soil scientists, despite his mannerisms, I had ever met. So I came back determined to promote him and make his station as inspector entirely clear. I got back to Washington about November 10.

At Dr. Knight's request, I had prepared a proposal on the Soil Survey for him to send to Eisenhower. But he sent it back saying that "we had no cause for alarm". Oh how he lead me down the garden path!

About November 20, J. Kenneth Ableiter joined the staff in charge of interpretations. I went to the Society meetings and we had much talk about the Soil Science Society. Unhappily the old "agronomy" heads got it established in a most ambiguous way - as both an independent society and as a division of the American Society of Agronomy. This was a big handicap to the Society for many years since it created the obvious inference that soil scientists had no interest except to application of the science to field crop production.

Beginning December 16, I started a course in the Graduate School of the Department on Soil Morphology, Genesis, and Classification. This came after hours three nights a week.

chapter 81a

The end of 1935 found me still negotiating with Milton Eisenhower over the Soil Survey vis a vis the SCS.

1936. We worked out an agreement on the soil survey that Bennett, Knight, and Wallace signed January 7, 1936, but I doubt that Bennett had any idea of observing it.

With the Supreme Court throwing out the AAA act, we had to get money for the work in Puerto Rico and Hawaii. At the end of the month, we got permission to include these amounts in a deficiency bill.

This sticks in my memory very well because the delegate from Hawaii called and told me that the Committee promised to include it if I would get it in the deficiency bill going up the next day. I really had to hurry. I carried it around the Department to get all the approvals including Wallace's

From time to time during 1935 I studied the problem of phosphatic fertilizers, much under the inspiration of Dr. H. A. Morgan, and realized that this had a great national significance for a permanent agriculture. I discussed this problem with Dr. Knight and with others prominent in the Land-Grant Colleges in the hopes of having a joint public statement. Dr. Morgan was urging a joint committee between the Land-Grant Colleges and the Department. This came into being the following year and a good factual statement was prepared for public release in October, 1936.

and then went over to Col. Dasher's office. He was out to lunch so I had to wait a few minutes. When he came back I explained the matter to him and he opened up and gave me unshirted Hell.

"You fellows get a big appropriation," he shouted, "and the first time some little thing happens you run over here for money. And the Treasury hasn't any money; its all borrowed."

For some reason this made me mad. I said, "Yes, Colonel Dasher, we get the money after we've justified every one lead pencil to the Department budget committee, to you folks in the Bureau of the Budget, and the two committees in Congress. And as soon as a bill is signed everybody along the line pressures us to do extra things that weren't provided for. I'm an S.B. if I don't do them and I'm an S.B. if I ask for money. I'm getting god damn sick of being an SB on both ends of the line. Right now I don't give a damn whether you send it up or not. If you do I'll wire the boys to stay

in Hawaii and if you don't I'll wire them to come home. I've done my best and you can do as you damn well please. Good day Sir." And I left. (He sent up the deficiency. This is the only time that I ever displayed temper at an appropriation hearing and don't recommend it. But at least this time it worked.)

But in the meantime Rockwell Smith, in charge of the soil survey in Hawaii got nervous, left us, and went with the sugar station. So we placed Foster in charge and ~~sent over Pony Lesh~~. later arranged for Marlin Cline to go over.

For a while, I divided my time mainly between defending the Soil Survey against Bennett and working on the manuscript for the Soil Survey Manual. The class in the graduate school was concluded at the end of March.

In early April we had a meeting of all the people working on the soil survey in the Tennessee Valley at Chatanooga. Morale was very good. I went out to talk over mutual soil problems with the people at the University of Wisconsin and Iowa State College. I got more at Ames from the economists

(T. W. Schultz, et all) than from Percy Brown in soils.

May 28, I took the boat from New York to Puerto Rico. This was a restful trip and I took along a most interesting new book by J. R. de la H. Marett: Race, sex, and environment, a study of mineral deficiency in human evolution. In Puerto Rico I saw many examples of the effects on children. (For some time I had thought of a study of the nutritional value of food and feed crops in relation to the kind of soil and its management. When I returned I sent the book to Secretary Wallace and he became quite excited about the general idea, which I was able to get started a year later.)

Ray Roberts had a big problem. Many of the ~~old~~ soil maps ~~done~~ made earlier had to be redone and the available photographs were poor. It was a shame that good air photos were not available. We saw many kinds of soil because of the great variations in elevation and climate. The island was beautiful San Juan seemed to be swarming with beautiful girls.

The survey was nearly finished so part of the crew came back with me. Fortunately, I had an opportunity to look over the University laboratory. I could see that their results could not be used. Later Mr. Roberts came back and I worked closely with him while he wrote the report in Washington. He did a good job.

I was back on June 15 and took another short trip to the Tennessee Valley. Shortly after, Lucille and the children went to Ionia.

insert 85a

On July 10 I started a long inspection journey in a government car.

First I looked over a soil survey in Clinton County, Michigan with Prof. Veatch, Johnsgard, and Striker. Then I drove by my old home and picked up Lucille and we started west, through Iowa and Nebraska to Akron, Colorado, where I spent a day with "Dobby" Knobel, an old associate from McKenzie days. Things were going well. And we went on west. It had been terribly hot up to now. It seemed very good to

Our program in the Tennessee Valley was coming along fine. On most of my trips to the Valley I spent as much time on the test-demonstration program and the general phosphate program as I did on the Soil Survey.

For some reason that I never did understand, Charles J. Brand, Executive Secretary of the National Fertilizer Association and editor of The Fertilizer Review, took a violent and completely unreasonable position against TVA. He used The Fertilizer Review for vicious attacks on TVA that had no basis in fact. Actually the work of TVA was doing more to encourage the use of fertilizers than anything else going on. The test-demonstration farmers bought much more fertilizer from commercial sources than they had before, in addition to that they received from TVA at reduced prices for demonstrations.

poke around in the Alpine Meadow soils near snowbanks.

July 19 and 20 I spent in a soil survey near Salt Lake City being run by Francis O. Young - a very good man. This time I had a good Zeis camera, and got a lot of useful pictures of soils, farms and vegetation. After Salt Lake we went on to Berkeley, looking at soils and getting pictures. We got to Berkeley, California July 23 and I had some time with Macy Lapham and Charles Shaw.

On July 24 we left by boat for Honolulu with six pleasant days at sea. I rested a bit but did a lot of writing too on the Soil Survey Manual and parts for a new book.

We reached Honolulu July 30 and left the next afternoon for Hilo on the big Island of Hawaii. In the meantime, I looked at the soils near Honolulu and visited both the Sugar Station and the Pineapple Station. Everyone was very helpful.

I recall that we had a nice room in Hilo and Lucille spent much of her time on plantations. The soils were very

interesting and are well covered in the published Soil Survey of Hawaii. I got many useful pictures. Many of those from this trip found their way into the Soils that Support Us.

We had a most useful time in Hawaii and stopped on the Island of Maui for one day on our way back to Honolulu. Lucille staid there while I spent August 14 to 18 on a trip to Kauai - the "Garden Island". We left Honolulu August 22 and reached Berkeley again August 27. This had been a pleasant trip on a luxury liner - the Lurline - with some rest and quite a bit of writing.

While we were steaming east, Marlin Cline and his bride were going west to Hawaii.

On August 28 we drove down to Bakersfield with Lapham and Shaw to look at the soil survey there. I went out with Retzer and the others to look at the Fresno and other soils. It was terribly hot. On the way back to Berkeley the next day I saw a Chernozem from granite with a slight C_a horizon - Marbut's "pedological line".

On August 31 we started for Corvallis, Oregon, with Macy in another car. We were met at Corvallis by Nikiforoff

and Wheeting. We looked at the Dayton soil that Niki was working on and then on through Everett, Yakima, and Walla Walla, Washington, visiting soil surveys and looking at the main soils.

On September 12 we went to Black-foot Idaho. I staid up late that night to read some soil-survey manuscripts sent to me - those years I read each one in manuscript and galley proof. I became ill about 2 o'clock in the morning and had to stay in. The next day Lucille drove and I went on, with bad cramps, to Livingston, Montana. The next day was better and we drove on through Watford City and Fargo and Iron River, Michigan, to Ionia. I staid there a day and drove back to Washington by September 22.

After I got back from Hawaii I found that SCS had tried to get James Thorp in direct violation of the Secretary's ~~ofers.~~ orders. But I promoted him. Our budget hearings went fairly well this year.

I was now putting the last touches on the Soil Survey Manual and sent it forward. ~~But~~ SCS kept it for a long time on the usual circulation, but no real trouble developed.

Insert 89a

I had a bit of work with TVA about the phosphate program and about extending the excellent cooperation on the soil survey to fertilizer research. But the fertilizer folks had been so "brain washed" with the propoganda against TVA that they wanted no part of it. (What a chance for greatness they missed.)

About this time I wrote up a statement for a project on the relationship of soils and food and feed quality. Dr. Knight agreed to support a request for special funds.

Frank O. Youngs had written to the President opposing one of Ickes' irrigation proposals. The project was looked into and found to be unsound. But, of course, Frank should have written through channels. I sent him a letter of reprimand and next day put forward the papers for his promotion. Yet, by being willing to step out of line he saved large public costs and misery to hundreds of innocent settler families.

This year the Soil Science Society meetings were in Washington. We had a meeting also of the inspectors of the Soil Survey.

I left the SCS "wars" for a while and went with Hearn on a trip to look at the soils of the south. We drove through Virginia, eastern Tennessee, western North Carolina, Georgia, Alabama, Florida (to the Everglades) and back through eastern Georgia, South Carolina, North Carolina, and Virginia. I got a lot of good photos and improved my knowledge of this area no end. We met a lot of the soil surveyors and state experiment station people. Near the end I bought a mouldy Virginia ham for Christmas dinner.

Again on December 14, I started the soils course in the USDA, which ran on to the middle of April with a few gaps.

Now the bad split in the TVA Board between Dr. H. A. Morgan and Lilienthal on the one side and A. E. Morgan on the other was about to become public. What a pity, but A. E. was no cooperator.

At the year's end I went to Syracuse, New York, for a meeting of the Association of American Geographers and gave a paper, Soil and People, which was published in this ~~Journal~~ annals in 1937.

1937. The Secretary, Mr. Wallace, insisted on having a Yearbook on soils. I tried to talk him out of it because of the confused state of the field, but he insisted. Eisenhower did agree to have Knight as chairman (which meant Kellogg on the scientific part.) Gove Hambidge was a good editor and we all went at it. During the next year and more I was to spend much of my time on this job. (The Manual was done, thank goodness.) Bressman and Hambidge had a row over the Yearbook outline in which Bressman lost out.

Thus with the Yearbook, defending the Soil Survey against SCS, working with TVA, and my class I had my hands very full. About this time I started to become acquainted more with Dr. John D. Black. I think I learned as much from him as from any other man.

One day in winter, I met Wallace in the hall. "How is your project on soils and nutrition coming", he asked.

"Not at all", I replied.

"Can I do anything", he asked.

"You certainly can, sir. Just call Dr. Jardine and tell him to approve a little project to start it that I submitted months ago".

The next day Jardine called me over to discuss it. And it was approved. I cautioned Knight not to put it under me formally but under Dr. Browne, else Byers would be distressed. (He was anyway.) But we got it going. Dr. Knight took my recommendation of Kennth C. Beeson, a P-2 in the Fertilizer Division who had done very well in my soils course, to be in charge as a P-4. Beeson worked diligently and thoroughly on the literature in all the European languages and assembled a most impressive bulletin that laid the basis for many other scientists. (The mineral composition of crops with particular reference to the soils on which they were grown. Misc. Pub. 369. U.S. Dept of Agriculture. 164 pp. 1941

Insert 1937

May 21 and 22 I went to the University of Michigan at their invitation to attend a round-table conference on research in land utilization. By this time Schoenmann had left TVA and was Pack professor of wild land utilization in the School of Forestry and Conservation.

The conference was organized in a very formal way with about 25 or 30 participants, outlining in a sort of horseshoe pattern with a head table. The junior staff and graduate students sat around the room. For the conference I prepared a brief statement on the organization of data about land in order to have a basis for rural land classification that would make possible the synthesis of other research data important to land use.

My chief recollection of this interesting conference was a rather vivid display of arrogance by Dr. John D. Black. (Later I learned to accept this for I don't think I learned more from any other man.). One of the participants was Elwood from the Iowa State Planning Board who was, by profession, a landscape architect. For sometime we had been discussing how to compare the dollar value of land used in different ways. Of course, one can estimate the inputs and outputs under various kinds of farming or forestry and get at approximate land values by the usual method of capitalization. But if some social use like recreation is in competition for a tract of land with farming

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one cannot use this method. ~~The man from Iowa spoke up and~~ Elwood said that they had found a way to do it in Iowa. They placed a value of 25 cents on each visit to a park and then used that as a basis for capitalization as you would ~~a~~ the total value of corn production. Dr. Black was in the chair and interrupted the speaker with this: "Professor Elwood, I must stop you right there or have nausea here in the room." He immediately turned to someone else and said, "Professor have you anything useful to say on this this point?" Of course, Elwood was pretty sore and so were our hosts at the University of Michigan. But this was John D. Black when people were talking nonsense.

One of the most difficult chapters of the 1938 Yearbook, Soils and Men, to ~~conclude~~ complete in final form was the one finally called The problem: The nation as a whole, which had multiple authorship. Roy Simonson spent days on it. The careful reader will note some of the early SCS propaganda about the extreme hazards and losses and a part of the other side of the story.

In the article the reader can find a table showing the annual losses of plant nutrients and estimates of erosion. Yet also, for the first time, a table is published giving the percentage of various plant nutrients by 1-foot depths. It shows that after truncation the nutrient content of the new surface soil may have either more or less plant nutrients, depending on the kind of soil and the particular nutrient. In the "balance sheet" prepared by Lipman of New Jersey and his associates all erosion was counted as "loss" of nutrients as if the material beneath, which becomes the new surface soil, were totally lacking in plant nutrients.

State committees of both state and Department people

were asked to make estimates of the potentially arable land available. Despite the great flood of propaganda about erosion, the table (page 95 of Soils and Men shows a reserve of unused soil suitable for cultivation in continental United States of over 100 million acres!

Yet in the minds of many, Bennett's idea of terrible devastation and of soil scarcity prevailed. The propaganda was enormous. Many were accepting Bennett's guesses as "data" for writing scare books to make money. In the days of the New Deal, somehow the acceptance of his figures about soil damage and need for enormous public action got identified with "liberalism".

On this question of soil scarcity, many economists found themselves on Bennett's side - not students, such as John D. Black and T. W. Schultz - but many other economists. Such economists ~~so many of these~~ are really historians and see only where we have been. Most had no appreciation of the great increases in production from (1) better management of soils already

being used and (2) the potential for bringing into use many millions of acres that had not been usable under the old systems of management.

It ~~These errors~~ bothered me ~~greatly~~. Great opportunities did exist for improved soil management. But the main reasons for such efforts was improved efficiency and income in a highly competitive agriculture and not primarily erosion control or scarcity of soil.

~~To make matters worse,~~ These same false ideas of the soil situation continued to be held for some 20 years by many people. Even worse, these generalizations were applied to the world by people who should have known better as well as by the scare writers and birth-control extremists. For the next 25 years it was destined that I should devote much time to the study of world soil resources and to simple explanations of their implications. Continually the backward-looking, historical economists continued to confuse and mislead almost as much as the emotional soil conservationists.

The SCS worked up a plan for soil conservation districts, at first mainly by watersheds. Legally, this was modeled by Phillip Glick after the old drainage and irrigation district laws. President Roosevelt sent out a "model" state law to each governor. Ultimately this worked out - an example of good out of bad intentions. Bennett used it to bypass the state colleges of agriculture, which he did. This finally stirred up the Land-Grant Association.

The Department agencies had a meeting at Mount Weather to discuss the possibilities for better cooperation. I attended this for 2 or 3 days in the middle of June. Milton Eisenhower held everyone's little hot hands.

In early July the Land-Grant representatives came in for a conference about the whole matter and July 4, 5, and 6 they had a joint conference at Mount Weather, which resulted in the famous "Mount Weather agreement" - a sort of treaty. This lead to the county-planning program in the BAE and to the appointment of Milton Eisenhower as Land-Use Coordinator

with a Liaison Board made up of representatives of the USDA agencies. Most were agency heads but I represented the Bureau of Chemistry and Soils. This was established August 5 and we had our first meeting that day. We met again August 12 and August 18. At that one we each agreed to furnish maps of the surveys made and under way.

Bushrod Allin did a splendid job with county planning and I helped all I could on his committee. But the farm organizations, especially the Farm Bureau, didn't want it. And SCS did not want to be coordinated. The Secretary should have had it in his own office, not in the BAE, which was one among many competing and jealous bureaus. Allin and I begged Milton to have representatives of the farm organizations and of the agricultural committees of the House and Senate on the advisory committee so they would know what went on. He refused. Possibly he really wanted it killed. Anyway SCS sowed seeds in the right places and the House committee on appropriations finally killed it.

Then years later Benson started the Rural Development Program and Freeman the Rural Areas Development Program with the very same purposes and similar approaches.

To continue with the Liaison Board, on November 24, I explained the work of the Soil Survey, which went very well. After the meeting old Dr. L. C. Gray came over and said how previously he had misunderstood our purpose and was sorry. Along about this time Dr. C. P. Barnes had been transferred to Eisenhower's office.

I can recall a prearranged meeting during which Eisenhower went through the motions of asking the opinion of the group about the SCS proposal of regional offices. Near the end he turned to me and said, "Now give your point of view".

I explained how the Department had traditionally worked with the land-grant colleges. We had encouraged farmers to look one place - to the college. Now farm leaders and congressmen would also look to the regional offices. They would be confused and the Department would lose control

of their own officers. I pointed out that this had already happened when one regional SCS man (Winters of SCS in the "Dust Bowl") had come to Washington, gathered together the Congressmen, and called in turn on Bennett, Wallace, and Roosevelt completely contrary to regulation. And no one has disciplined him yet. (They did later; they had to.)

I also pointed out that the analogy with the Forest Service was not sound. They worked on public land for which they had management responsibility, but the SCS worked on private land and both state and county officials had important roles.

But, of course, I was outvoted and those regional offices lasted until 1953, Bennett purposely put them far from the land-grant colleges except for Lincoln. The libraries and with facilities of the college should have been of enormous help to any country in their offices. Their ghosts of these old offices were in evidence for years after 1953. After Eisenhower went to Kansas State he used these same arguments of mine against the same SCS regional offices!

At another meeting we discussed the matter of having soil conservation districts on a county or watershed basis. The advantages of each were put forward. Dillon Meyer agreed

to let the local people have a free choice. But I learned later that on that very day wires went out from SCS to approve only those on a watershed basis. But the people would not stand for it in most places.

In August and September Milton Eisenhower and his staff beat me down to accept a new plan for a joint committee - at first Baldwin, Norton, and Barnes. This was approved by the Secretary September 25. But like the other agreements neither Bennett nor Norton had any real intention of following it. Norton so insulted Baldwin that after the first few months I had to serve so long as we had it.

The Soil Survey Manual came out in September. I had many compliments on this, but the nicest from Dr. F.J. Alway of Minnesota: "I wish to express my appreciation of the way in which you are getting ~~the~~ things done that have been in special need of attention". H.H. Bennett ~~wrote~~ thanked me for Manual and wrote! "I am very glad to have this ~~thing's done.~~ for my files". (He left it there!)

I knew the weak places and developed a plan to have large national staff conferences that would take up the biggest problem areas one by one: color, texture, structure,

and so on. Many committee reports were prepared, criticized, rewritten, and circulated for test over the next ten years.

Sometime in August, Dean Dana offerd me a nice professorship at the University of Michigan. Dr. Knight was kind enough to have my salary raised from $5,600 to $6,000.

In October, Bushrod Allin and I took the propaganda paper Bennett wrote for the Yearbook to Eisenhower. I said that I could see only three alternatives: (1) I resign from the Yearbook Committee; (2) Bennett get some one to rewrite the piece; or (3) I write one on the same subject. He chose the middle one, and we got a slightly better but still poor paper, probably by Lowdermilk or a press writer.

During October, November, and December, a Mr. Dallas Dart, of WPA asked me to pass on some WPA proposals put forward by J. G. Lipman of the New Jersey Station -- one on getting an "organic matter" balance of the United States and the other for a soil science dictionary by Joffe. I gave him confidential memos in opposition, with reasons. I was suspicious since Lipman had already published a ridiculous "nutrient balance of the soils of the United States." (Secretary Wallace got keen about this one, but I was able to show him in five minutes how absurd it was). But these memos were not

kept confidential.

During the first week of December I went to the Soil Science Society meetings in Chicago. I'm not too happy with the new arrangement but we shall see. Was also able to meet with the inspectors.

Insert 100a, 100b, 100c, 100d

With so much to do yet on the Yearbook and the faint possibility of going to Europe I did not give the course in the Graduate School this winter. I read carefully every article for the Yearbook and did much work on many of them.

Insert 1937

As I recall Mr. J. K. Ableiter presented his first paper on soil productivity ratings at the Society meeting in the autumn of 1937. It was a good paper but some of the old timers like Joffe and Krusekopf objected. I recall particularly Krusekopf saying that we should not deal with crop yields, but should define the basis for rating soils within the soils themselves, which was an absurd statement and he and I had some sharp exchanges on the floor. Then he took for the tall timber!

This whole subject had become a burning issue. Dr. Marbut had not taken much interest in these kinds of interpretations. With the help of Dr. C. P. Barnes he had attempted to make ratings of the "natural" productivity of soils, which were published by the National Resources Planning Board. I had to write many letters about these for years afterward. In that rating Iowa came out with an abundance of soil and California with very little. The more one thinks about "inherent" or "natural" soil productivity, the slippier the concept becomes. It became clear to me very early that one could make productivity ratings for soils only under physically defined systems of management. It had to be expressed in terms of yields. Then at this point the economist and soil scientist could join hands. The economist could calculate the value of inputs and the value of outputs.

I was determined to push this kind of soil survey interpretations and all other kinds for which we had data as soon as that one was well on its feet. Strangely I got enormous criticism from men

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like Joffe and Hans Jenny, and even from some of my own staff, including Thorp, Baldwin and Nikiforoff. Their idea was that the soil survey was an activity of basic science and to attempt these applications lowered its dignity.

Then on the other side there were people like Hugh Bennett and others in the SCS who said that soil surveys were no good because they were too scientific. They attacked me on the basis that I was interested only in profiles and the great soil groups. They wanted a simple practical map!

Many people had been working on interpretations including J. O. Veatch with his "natural land type." Although this concept had a bit of merit, J.O. never thought it through enough to make it sufficiently quantitative to have prediction value. J. O. got misled by taking Dr. Marbut seriously in his repeated statement that soils are mapped on the basis of their profiles. Of course, they never were. The profiles were used to define the soil landscapes or soil individuals. But the boundaries came whether there was a change in one or more of the factors of genesis. J. O. knew that the soils in Michigan as mapped had variations in their profiles so he did not want to call them "soil types" as Marbut had defined them. Actually J.O's land types are whatever he said they were when he was in the field. It turned out that part of them were phases of soil types, soil type, and some more soil complexes.

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One of the most illogical schemes developed was the so-called "Storie index." (See An Index or Rating the Agricultural Value of Soils, by R. Earl Stories, Calif. Agri. Exp. Sta. Bul. 556. 1933.) In this "system" Storie rated 3 sets of factors: A, profile; B, surface texture; and C, other modifying conditions. These rating were purely subjective but then he multiplied them together and this somehow was supposed to have made them objective. Storie had fair judgment on the general usefulness of soils under common practices. In personal conversation he admitted to me that he first decided the final rating or index and put the factors in afterward. But he would never admit this publicly. Soil survey reports for publication coming in from California always had these factors and index ratings. We did not publish the factors, but did publish his ratings since we had nothing so good. I recall one time that he made a mistake in multiplication and our editor corrected the index. I told her that she should write about this to Prof. Storie. He implied that he made an error in the factors and changed those so that the index came out as he had submitted it the first time. She was amazed but I knew this would be his answer.

For years one of the most difficult jobs I had was to keep a reasonable balance between the basic aspects of the work and interpretation. Continually I had two sets of extreme critics, those who did not want any interpretations and those who did not want any science. Subsequently several soil survey institutes in various

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countries went to pieces because of overemphasis on one or the other. The British Soil Survey and the Australian Soil Survey went very weak on interpretation and ultimately lost much or all of their support. New Zealand kept strong in both. Much later I summarized this problem in a paper given at the New Zealand Soils Conference in 1962, which is published in their transactions.

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1938. As the new year started also working on Yearbook, phosphate message of the President, and TVA cooperation. It was decided to name the Yearbook Soils and Men and dedicate it to Dr. C. F. Marbut.

The WPA people did not keep their word but told Dr. Lipman that I opposed their projects. Lipman then complained bitterly to Knight and Jardine about my "lack of cooperation".

One day during the last of December or early in January, Dr. John D. Black came into the dining room and sat down near me. He said, "I see you are going to Europe." I told him that was news to me. "You'll find out in a few days". Then he told me that I would go on the American Delegation the the meeting at the International Institute of Agriculture in Rome. ~~He~~ Dr. Black had been asked too, but couldn't also was said that Wallace did this partly in appreciation of my work on the 1938 yearbook - so J. Kenneth Galbraith, an instructor in economics at Harvard, would go for him. (Ken combined this with his honeymoon.)

~~About the same time~~ On January a session of the Commissions of the International Society of Soil Science on soil chemistry and

soil classification, as I recall, were announced for Finland at almost exactly the same time as the meetings in Rome - May 23 to 30, so I recommended that Dr. L. T. Alexander be authorized to go to Finland, which he was.

Our budget hearings went badly February 4. I also heard that with the move to get the big chemistry labs to find new uses for farm products which ~~that~~ were being advocated, the research soil might be moved out of the Bureau. Knight did not think so. Just the same I realized that Knight had done a good job in getting some support for real work in agricultural chemistry (or perhaps we should say industrial chemistry on agricultural products) without being committed to the extreme views of the "chemurgists" represented by Hale and Wheeler McMillan.

Outside of my usual short trips to Knoxville and for a speech or so I kept at the Yearbook, Soils and Men, and at watching my Manual. About April 5 I finally completed the Glossary for Soils and Men, even to Bennett's satisfaction.

~~Then~~ another visit I made on April 7 to Knoxville and talked with McAmis, Sessions, and Bass on USDA cooperation. ~~Then~~ In the afternoon I talked about the work with Eric Winters, L. R. Schoenmann (we had enticed him to join the staff some months before), Moon, Bushrod Allin, Moores, and others. We had a good session of people concerned with the soil survey work in the Valley during the next two days.

insert 103a

On April 12 I agreed to the SCS using Foster in Hawaii, if they would pay the costs. Assistant Secretary Harry Brown agreed to see Senator Russell about our appropriations. Then on April 14 McAmis, Sherman Johnson, John D. Black and I had a good session on economic analyses of test demonstration farms in the Valley. On May 3 we had a good hearing before the Senate Committee on appropriations. That night I took a sleeper for New York. On May 4 I left on the S. S. Washington for LeHavre.[1] Dr. and Mrs. A. G. Black and their misbehaved boy were along. Mrs. Black so worried that I would not were a tuxedo for dinner that she bribed my room steward to lay it out on my bed!

1/ Notes, photographs, and exhibits are included in Notes on European Trip (1938) and will not be repeated here.

Charles Brand of the National Fertilizer Association continued to put out ridiculous propoganda against TVA. Obviously he was getting help from K. D. Jacob and some of his staff. (Later I was to discover that he probably got a good deal from Frank W. Parker with the Dupont Company.)

On the boat I had 6 days to write and rest. I was not sure what I would do with the writing but part of it went into The Soils That Support Us.

May 11 we arrived in Le Havre and went directly to Paris. The Embassy people took us at once to the Louvre. Most of my days there were spent with Prof. Demâlon, Georges Aubert, S. Henin, and others of the staff at the Versaille Station and in the country about. Aubert was already translating the Soil Survey Manual into French. Saturday, May 14 and Sunday I had some good sight seeing in Paris and out ~~toward~~ to Rheims and the champaign area.

May 17 I left Paris by train and visited the experimental station at Avignon. I had a great treat in seeing the old laboratory of the Frenchman. The next day I took a fine visit to a perfumery factory and to the field where the flowers were grown on Terra Rossa soils. I went on to Cap d'Antibes, where the former Prince of Wales held out. Southern France in May is an inspiration. I went on to Florence - the most beautiful of cities - and on to Rome in the evening of May 22.

Heap: B. Bavergaint made the first people field experiments on soil fertility.

The Institute meetings were interesting. I first became acquainted with the mysteries of framing resolutions and got

into disputes with F. L. McDougal of Australia (who later became prominent in FAO). I tried hard, and so did our permanent delegate, J. C. Marquis, but I made no firm progress toward a world soil map, partly because the British would not agree to anything that cost money. I also met our famous Soviet agricultural expert, L. G. Michael. I was able to get some recognition, however, of the importance of soils in the debates on nutrition and a bit of this got into the formal resolutions as adopted. The resolution for a study of world resources, including soils, was passed but no money was provided.

The meetings left some time for sightseeing around Rome with Dr. and Mrs. Galbraith and trips to a consorgio in the hills and to the Pontine with M. L. Wilson.

June 2 Mr. and Mrs. M. L. Wilson and I go from Rome to Naples. (Mr. Wilson as Undersecretary and Head of the Delgation got my authorization changed to accompany him to Greece and through central Europe.)

We went up to the crater of Vesuvius. For some reason I tried to make a panorama of the harbor at Naples by overlapping shots with the camera levelled each time. It turned out well and our military intelligience were very grateful in 1943.

We went by train to Brindisi. Here we had to check out at the local police station. M. L. forgot to "remove his hat" according to signs in several languages when we went to get our exit permits. His puzzled look at the Italian-speaking officier so amused me that I could hardly get through to M. L. in English. When I did, he removed his hat and got a big smile and his permit.

Then we went by boat to the Athens port. We visited old Corinth. In one day we went out by taxi to the Temple of Apollo and back for dinner at our embassy with the King of Greece. Only this time did I literally pinch myself to keep awake.

On June 10 we travelled to Salonika and spent two days at the American Farm School. We had two evenings and one day in Belgrade and then on to Budapest. Our time was running short but we did have a fine visit at the great agricultural museum of Hungary. and I had a nice visit with Prof A. A. J. de Sigmond. War seemed imminent and the Hungarians weren't sure which side to be on.

We arrived in Vienna on June 16 and had but little time there. M. L. went on to Prague in the evening and I the next morning. This was just after the Anschluss and this normally gay city was very quiet. I shall never forget the coarse, insolent woman who permitted me to pass to the train. She was like Dickens' Madame La Farge with a swastika armband and a 45 on her belt.

Things were very jittery in Prague and I went on to Berlin with some trouble over my hotel which I reached very late. We had a good visit to a farm about 60 km. north of the city. M. L. and his wife went on to London while

I visited Professor F. Schucht, his staff, and the laboratories in Berlin.

On the street later I stopped a man to ask the way. He turned out to be a German farmer on his first visit to Berlin. I explained that I was an American agriculturist. We sat down and had a good chat. He showed me pictures of his farm, his prize animals, and his family. Whereas I had had trouble talking to the professors (I simply let the "ders, dies, and dases" take care of themselves) I had no trouble at all with this man.

I was crudely under surveillance all the time. The painting and breaking up of Jewish shops was evident on the main streets. Hitler made a big speech at the Sports Arena and I got there just as it was concluded and the fat German burghers with the big swastika armbands were drinking beer to the glory of Hitler and the Reich.

Rather carelessly I had agreed to take M. L.'s baggage with me to Hamburg for the boat. Ne and Mrs. Wilson went on to London for a day and would get on at Southhampton. There were some Jews agead of me at the barrier. The customs people were making them partly undress and were shaking all of their bags empty. When they finally got onto the boat under the American flag I have never seen greater relief. When my turn came I was worried because I had picked up lots of things to take home and had no key for M. L.'s bags nor any idea what they contained. In my best German I explained the matter to the customs man, who understood me but scolded me for my bad pronunciation.

He simply marked the bags and called a man to carry them on the boat. Then he held out his hand and said, "you'll have no further use for your laissez-passer." This the German ambassador had given me in Belgrade and I had shown it to no one in Germany, nor had I told anyone at the hotel where I was going when I left. But he knew. I, too, was relieved to get under the American flag. It was perfectly obvious that Hitler would start a war.

I left Hamburg on the S. S. Harding at 6:00 a.m. June 22. We had a few hours in Le Havre — with time enough to get a basket of liqueurs — and then on to Southhampton and New York. We docked the afternoon of July 2 and got to Washington late in the evening.

When I got back to the office my staff told me that everything was fine in the Soil Survey (which it was not.) ~~I tried to catch up with office work.~~ ← insert 109a b, and c

On July 16 Lucille and I started out for Knoxville and did a little sightseeing on the weekend. I gave 11 lectures at the University, ending July 23. In the afternoons I had

Because of the 1938 weekend holiday I had some time at home.
(About July 5).

Perhaps the most rewarding part of this trip to Europe was my association with M. L. Wilson and his wife. He took along quite a few books and part of the time that we were traveling he insisted that I read to him. I recall reading to him most of the way on the train from Naples to Brindisi and some on the boat to Athens. M. L. was interested in everything and we talked a great deal about the relationship between soil and civilization. He had been impressed with Walter Lowdermilk's writing and I explained to him how inaccurate ~~it was~~ they were because Lowdermilk took no account of the effects of the many ~~series~~ works in the Mediterranean Basin and the Near East. Further, he took no account of the great changes in climate during the past few thousand years. At one time the Sahara was bigger than it is today but while the ice stood over Europe it was mainly covered with grass. Then when the ice receded it became dry again and much of the movement of soil resulted from this thinning of the vegetation. The irrigated areas were often abandoned for long periods during ~~the~~ wars and, of course, the channels silted as they did elsewhere.

During some of these conversations M. L. was a bit doubtful but he got a good shock at old Corinth. We stood there looking at the excavation. In fact, I got a fine picture of M.L. standing in the approximate spot where St. Paul stood when he preached to the Corinthians. He said to me, "Now Charles you have got to admit that here was a place that was covered by soil erosion." I asked

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him, "Do you mean the erosion of cropland?" He said, "Why yes." Then I asked him, "Now M.L. we have been looking at several farms in this area, what color was the soil on these farms?" He stopped and thought, then he said, "Why the soils were red." (They belong with the Terra Rosa group.) So I turned to the excavation and I said, "Look, M. L. what is the color of the earth these men are digging out to find the floors of the old buildings?" He looked and looked, and then he turned to me and said, "Why it isn't red at all it is a sort of yellow-gray."

Then I directed his attention to the mountains and the whole series of alluvial cones characteristic of the margins of mountains in dry areas. I told him he could see these same phenomena on all the ~~our western~~ mountains in the semi arid parts of the United States ~~dry areas~~. The water rushes from the top and gets concentrated into natural gullies and they spill ~~this~~ out on the margins and drop the rock debris. Between two of these here in Corinth there was a spring with nice water and the ancient people built their town very near ~~right next to~~ the spring. Its destruction was inevitable. As the cones built up, finally a new gully appeared between them. This always happens and nobody knows how to prevent it in a dry country. Thus at the very moment they built old Corinth its doom was sealed without any significant relationship to land use.

We had many other examples of these phenomena in Greece especially along the train from Athens to Salonika ~~Solinka~~.

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On the boat going home we had many hours of discussion about this relationship between soil and people as affected by the geological processes, the development of new technology, and wars and revolutions. M. L. insisted that he and I should go to Mexico in the dry area and study these relationships in relation to the old and present cultural there. (But we never found a chance to do it.)

sessions with Moon, Winters, Bass and the others. As I recall now I left the examinations questions and Mr. Schoenmann, who still worked some for TVA, corrected the papers. We returned to Washington by way of North Carolina and Virginia and it rained practically all of the way.

July 25 I had more conferences with Eisenhower, Baldwin, and Thorp about the difficulties with SCS.

We had a curious discussion about water facilities on the Liaison Board. It was bound to fail but I didn't dare say so. BAE is to do the planning; SCS is to build the structures; and FSA is to handle the money!

The next day I started the business of getting Baldwin's grade raised. I also learned that Knight and Eisenhower had recommended my promotion to P-7 and I would have the soils work and the fertilizer work.

From about July 30 to August 6 I joined a travelling conference on soil correlation beginning at Minnesota and

ending at St. Louis. Many of the state experiment station people were on all or part of the trip to improve mutual understanding of the soils. My most vivid recollection was the enormous heat in St. Louis. Mark Baldwin was along and neither of us slept ~~a bit~~ at all so we changed our reservations ~~a bit~~ one day and headed home on the afternoon of the 5th of August from East St. Louis. We simply sat for an hour or two in the train to absorb the coolness of air conditioning.

The next day I told Dr. Knight that the rumor was that the soils work would be moved to Plant Industry. He told me that he wasn't disturbed.

A few days later Knight called me in and said that M. L. Wilson wanted me to go with him to study the Indian civiliation in Mexico. He thought I should do it. M.L. also told Knight that he wished I would write something to spike this bad material that Lowdermilk was putting out.

The next day Knight talked with Eisenhower about the rumor of the soils work going to Plant Industry. But he

told Dr. Knight that they were thinking of moving agricultural engineering into his Bureau. He even told me to quit worrying! (Oh how Milton Eisenhower could lead them down the garden path!)

(1938)

About August 14 I take a government car and make a visit to our soil survey work going on in New England. W. J. Latimer went ~~most~~ much of the way with me but not all ~~the way.~~ it. We went through the Connecticut Valley and I had my first good look at tobacco growing under shade on the Enfield very fine ~~silty~~ sandy loam. We spent some time with Mr. Sherwin who was completing the soil survey of Rhode Island, then we went back into Massachusetts and took a brief look at the soils on Cape Cod. ~~Then~~ We went on up into York County Maine, examining the soils on the way. This gave me my first opportunity to see the famous Cadillac Mountain on Mt. Desert Island. We ~~went through~~ looked at the site for the famous Passamaquady Power Project. (Later this fine project was killed in the Senate through the influence of the power company in Maine.) We drove North ~~quite close to~~ to the New Brunswick border and found some most excellent Podzols. One of the photographs taken there of a soil in the partly-acid Brasua series, has been reproduced in a great many publications. We continued on and spent some time looking at the agriculture and soils around Presque Isle. The prominent soil here is Caribou loam developed from somewhat calcareous Late Wisconsin till. The best of this soil furnishes the basis for the ~~great~~ prominent potato ~~production~~ farming of Maine.

Latimer left me here and I went on alone through the deep Maine woods a little bit below the Canadian border. This was about as wild a country as anybody could hope to find. I stayed in the first village in New Hampshire and continued on

[illegible] out figure 1[illegible] I cube a [illegible] and make a [illegible] soil survey work going on in [illegible]. [illegible] J. Latimer [illegible] of the way [illegible] but was [illegible]. He went [illegible] Connecticut Valley and I had my first look at tobacco [illegible] under shade on the [illegible] farm. We [illegible] some time with Mr. Austin who was supervising the soil [illegible] of Rhode Island. Then we went back into Massachusetts and [illegible] a brief look at the soils on Cape Cod. Then we went on up [illegible] York County Maine, examining the soils on the way. This [illegible] me my first opportunity to see the [illegible] Penobscot [illegible]. Desert Island. We made trips in the famous Aroostook [illegible] project. (Incidentally in the 5-mile [illegible] through the Indian [illegible] the power company in Maine.) We [illegible] with [illegible] to [illegible]

[illegible] the photographs taken from [illegible] on a soil in the Aroostook section [illegible] been responsible in a great many publications. We continued [illegible] and spent some time looking at the [illegible] soils around [illegible]que Isle. The prominent soil here is Caribou loam [illegible] [illegible] somewhat crumbsons) the [illegible] till. The best of this [illegible] furnishes the basis for the famous potato [illegible].

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across into Vermont. I stopped at the University and then came on south through Concord, New Hampshire. Here I took a picture of myself with a timing mechanism that was widely used later in advertising The Soils that Support Us. In fact, ~~many~~ several of the pictures taken on this trip appear in that book. We had a soil survey of Hillsboro County and I spent some time with the ~~boys~~ party, ~~and~~ including Charles Simmons.

I went on over to look at the soil survey of Dutchess County in New York with Dr. Bradfield and Mr. Latimer. This survey was made with a very poor legend. The legend had been developed from examining roadcuts and the cleared landscapes with altogether too many phases that the boys were unable to map in the woods and brush. This had been cooperative with SCS and they were looking for erosion phases and all that sort of thing. I never saw Dr. Bradfield more put out in my life. (The only way we could make a survey out of this was for Gordon Johnsgard to spend many months going over it and making a reasonable new manuscript map that would be publishable). I was back in the office about August 31.

I had more writing to do, including a paper for Collier's National Yearbook and an important paper for the Society meetings - Recent trends in soil classification.

It amused me no end that the SCS regional people were kicking about the complexity of the "soil conservation surveys" with the uncontrolled legends. And their hand books provided no basis for classification, only for taking notes on a map. With this lack of system, the SCS set up arbitrary classes of slope, classes of "erosion" (in the sense of eroded soils), soil types, and so on. The "erosion" classes were numerous and complex. By allowing the mappers, most of whom were inexperienced and working without scientific guidance, to map any combination of these classes, it was common to have 3 to 10 thousand different mapping units in one average county where a reasonable classification for a detailed survey would have provided 75 to 150 mapping units. No one could make any sense out of such masses of insignificant separations.

Instead of straightening out the logical errors and developing a proper system of classification, the boys met the problem by what was called a "land-use capability" system, later shortened to "land capability". This was done to get away from the term "soil grouping" which it was. And the irony of the term is that they got it from some of my papers where it was used in a different sense. Naturally, I had a great deal of ribbing about this for several years.

The system did help to give the farm-planning technicians something they could use. But it avoided classification and soil study. Most of the field workers making farm plans would have cut out the survey but Bennett insisted that every farm have a "soil conservation survey". For this he deserves credit. But he could not bring himself to cooperate with the Soil Survey and develop a classification and set of interpretations so that the map would be really useful. This had to wait for nearly 15 years.

Actually the basic assumptions of the system were not thought through and compromises were arrived at by a sort of average judgement based on empirical experience, mainly in the central part of the Mississippi Valley. Thus the least erosive arable soil used for field crops unconsciously became the top standard. Not until about 1958 were the assumptions stated and the application only to field crops pointed out. Quite different groupings have to be used in range, forestry, and horticulture. Yet Bennett insisted that the range men use it, which they obviously could not. So they developed another quickie scheme called "range sites" that also lacked any firm scientific basis.

As a further confusion the term "land capability survey" came into use. Many people got the idea that the capability units and classes were mapped directly. Bennett's own writing encouraged this. In fact, he said and wrote that this system was the greatest invention since the wheel!

It was a shame that so many young men spent so many days of hard work in the field and produced so little of lasting value because of deficiencies of scientific guidance due to the bureaucratic arrogance of one very ambitious man.

~~1938~~

On October 5 Dr. Knight was called in to Mr. Appleby's office - he was Assistant to the Secretary - and told that the soils work of the Bureau of Chemistry and Soils would be moved to the Bureau of Plant Industry. I had heard it only by rumor. That night Dr. Knight went to New York and called me in on October 7 to tell me about what was going on. He and Dr. E. C. Auchter, Chief of the Bureau of Plant Industry, had a conversation that day and Knight was told to stop all personnel actions. This included mine that was in the Commission at the moment.

I remember three or four conversations about this matter at the time with Milton Eisenhower. He showed me the first draft of the order that mentioned my name as assistant chief of the Bureau of Plant Industry for all the soils work. Auchter had told him, so Milton said, "This is all right, but wouldn't it be better to leave it out and let me make the announcement?" Again I was led down Milton's famous garden path. He also told me that Dr. Knight had originally insisted that I remain back with his bureau to help him organize the new chemical laboratory. (I was at that time working up alternative flow charts and organization plans. The only good thing about the transfer was that I was relieved of that job.) But apparently both Milton and Appleby pointed out that this would hurt my career and certainly Dr. Knight did not want to do that. Dr. Knight had also tried hard to get all the research in SCS grouped with that going to Plant Industry, but without success.

[illegible] Dr. Knight was called in to Mr. [illegible]'s [illegible] - he was Assistant to the Secretary - and told that the [illegible] work of the Bureau of Chemistry and Soils would be moved [illegible] Bureau of Plant Industry. I had heard it only by rumor. [illegible] Dr. Knight went to New York and called me in [illegible] to tell me about what was going on. He and Secretary [illegible] [illegible]ction that day and Knight was told to stop all preparations. This involved mine that was in the Immigration [illegible] the [illegible].

I remember there on the conversations about this matter [illegible] time with Milton Eisenhower. He showed me the letter that [illegible] order that mentioned my name as assistant under [illegible] Bureau of Plant Industry for all the soils work. Milton [illegible] so Milton said, "This is all right, but we don't [illegible] to leave it out and let me make the [illegible]" I was led down Milton's famous garden path. [illegible] Knight and [illegible] that I [illegible] to help him organize the new Chemical Laboratory. [illegible] at that time mounting a alternative [illegible] and [illegible] plans. The only good thing about the [illegible] that I was relieved of that job.) But apparently [illegible] [illegible] pointed out that this would [illegible] Dr. Knight did not want to do that. So Knight [illegible] tried hard to get all the research in soil [illegible] [illegible] to Plant Industry, but without success.

The actual date of the transfer was October 16, 1938. This became very complicated because the Bureau of Chemistry and Soils had a centralized administrative unit while in the Bureau of Plant Industry each division had its own unit. So immediately I had to set up such a unit and a file unit. Then too, Auchter was worried about my title because all of the division leaders in Plant Industry were called "Heads." I think due to Allanson and McCall he was persuaded to leave my title alone and for years I remained the only division chief in that bureau.

Fortunately, during the work on the Soils Yearbook I had become very well acquainted with Dr. M. A. McCall, Chief, Division of Cereal Crops and Assistant Chief of the Bureau. He was one of the finest men I ever met in the Department of Agriculture. But Auchter was jealous of him because of his fine relations with the State colleges.

These became very trying days because I had a lot of work on hand, including the continuing hassles with Hugh Bennett, my work with TVA, and so on. TVA had been trying to get me to come with them and Dr. Knight advised me to do it. I considered it but I knew that if I left that would be the end of the American Soil Survey as Dr. Marbut and many others had built it up and conceived it. And I knew that the State colleges would continue to support me if I did not weaken or compromise.

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Auchter was a strange man. He worked very hard. Every night he took great ~~gobs of things~~ sheafs of papers home in his little satchel. And I soon found out that he lost many of these and would then claim we never sent them. So I had to put special slips on everything that went to his office and somebody had to sign for them before my messenger could leave them. Auchter never disclosed his plans ahead of time. When his announcements came out they came as a complete surprise to practically all the staff. The morale I found was much lower than in the old Bureau of Chemistry and Soils. No one knew what Auchter would do.

I never worked with a man so jealous of his staff as Auchter was. If any man in the organization got any kind of an honor he could be sure that within a week there would be something in his incoming basket to embarrass him. It might be simply a refusal for no reason to promote some junior scientist whom he had never seen or knew anything about. I had at this time many relations with the Secretary's office of which he became very suspicious. M. L. Wilson had told me, and I explained it to Auchter, that he wanted me to leave the summer of 1939 open because he had some special work for me to do, but did not tell me what it was. The House Appropriations Committee had been very critical of the Department for not finishing their research project. A special committee had been established to make a reply to this including

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the Bureau chiefs with Jardine as chairman and with me as executive secretary. Jardine did not do anything on it and Appleby looked to me to get the report. We had many sessions and I finally developed a report and a young man from the Department's Budget Office and I had to sell it to the Bureau of the Budget.

I am sure that the biggest mistake I made was over the fertilizer work. Auchter and I talked about this and he said that it ought to have been transferred with the other soils work, with which I agreed, but he did not want to ask the Secretary to do it; so I explained it would not be difficult to get it done if the Secretary should write a letter to Dr. Jardine asking him to make a review of the situation and come up with a recommendation after discussing it with Auchter, Knight, and others concerned. He said, "I could not get such a memo signed." Very naively I volunteered that I could get the letter signed. So we drafted the letter and I took it over to Milton Eisenhower and he got it signed. This deepened Auchter's suspicion that I was a dangerous man and all I was trying to do was to be helpful.

Shortly after the transfer he asked me, "What are the two most important pieces of soil research that should be emphasized?" I told him that the problem of phosphate fixation in soils was one and the work we had just started on the relation of soils to the quality of feeds and foods was the other. He asked me to write up a memo for him on these two which I did with the help of other staff members, especially Beeson and Alexander. He thanked me and that was that. Then one day quite a bit later he called me over

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and handed me a paper, and said, "I wish you would take this in a little office and read it through rapidly. Then bring it in and we will discuss it." This was a paper that he was to give as chairman of Section O of the AAAS. It was based on the material I had given him about soil and food quality. When I brought it back he said, "What do you think of it?" I replied, "There are two major points that I should like to call to your attention. First of all, you have ~~got~~ a bit too much about soils in here. Won't some of your friends in horticulture raise their eyebrows a bit since you have not worked in this field?" He agreed with this. Then I pointed out that there was quite a literature in this area and some of the pioneers should be given a bit of credit. I started to name some of the books and he said, "Wait a minute." He called in his secretary and told her to take down the feferences that I gave him and to get them out of the library. I don't recall now that I saw the paper again, although I may have. I found out much later, however, that he was working up a budget for a new laboratory to be located at Cornell. For this he talked to neither Beeson nor ~~I~~ me and we ~~did not know~~ knew nothing about it until we read it in the House hearings!! But he got it. This reminds me of another way he put me in my place. As long as I was in the Bureau of Plant Industry, through 1952, I was given no opportunity to defend the Soil Survey budget at the hearings. It is a wonder we didn't go under because

He told me once: "It is bad to discuss your plans in advance. This gives time for opposition to build up." But ~~[illegible]~~ when powerful in his Bureau decided anything could happen.

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[illegible] me a paper, and said, "I wish you would take this in [illegible] little office and read it through rapidly. Then bring it in [illegible] will discuss it." This was a paper that he was to give [illegible] man of Section C of the AAAS. It was based on the material [illegible] him about soil and food quality. When I brought it [illegible] he said, "What do you think of it?" I replied, "There are [illegible] major points that I should like to call to your attention. [illegible] or all, you have got a bit too much about soils in here. [illegible] some of your friends in horticulture raise their eyebrows [illegible] you have not worked in this field?" He agreed with [illegible]. Then I pointed out that there was quite a literature in [illegible] and some of the pioneers should be given a bit of credit. [illegible] started to name some of them[illegible] he said, "Wait a minute." [illegible] in his secretary and told her to take down the references [illegible] I gave him and to get them out of the library. I don't remember [illegible] I saw the paper again, although I may have. I turned out [illegible], however, that he was working up a budget for a new [illegible] to be located at Cornell. For this he talked to neither [illegible] nor I and we did not know about it until we read it in the [illegible] hearings." But he got it. This reminds me of another way he [illegible] in my place. As long as I was in the Bureau of Plant Industry, [illegible] 1932, I was given no opportunity to defend the Soil Survey [illegible] the hearings. It is a wonder we didn't go under because [illegible]

none of the people that went to the hearings knew much about the work. I gave them every kind of statement that I could, but anyone who has testified at hearings knows the impossibility of anticipating all the questions that may be asked. For example, I remember ~~that~~ the back in the earlier days when Mr. Cannon was chairman of the House Subcommittee for Agriculture, most of the hearings was used to talk about liming. One of the members had a farm and here was a chance to find out about this subject. Had I not known about it, my appropriation would have been in jeopardy even though this had no direct relationship to the Soil Survey.

~~But first of all,~~ From my standpoint, Auchter was the cleverest and most successful ~~liar~~ juggler of truth and falsehood that I have ever met. Of course, we all have occasional ~~liars~~ people of this sort to deal with but he was one of the few smart enough to be able to remember what he told different people. Most folks can't do that, but he could. I saw him in operation on several occasions that I knew about. Some subject would be coming up for interagency discussion, and he would call various ones of us in ~~and to~~ get himself thoroughly briefed. Then when the meeting came he would profess complete ignorance and get away with it. I saw several times the lines he put in front of Auchter who knew the facts.

In October 1938 I spent a day or so at Harvard with John Black and Ken Galbraith who gave a seminar for the famous "Land-Use Seminar". The damage of the September hurricane had been terrible in many places.

In mid November the meetings of the Soil Science Society and Agronomy Society were held in Washington. It was a very busy time. My paper turned out to be an important one but more important were my conversations with the leaders in the land-grant colleges. I had almost universal expressions of support. Of course you never know about these things but I am sure that Salter, Bradfield, Throckmorton, Pierre, and many of the others were completely sincere.

This raised my morale considerably and I realized that I had to fight to win. If I could have found a way to compromise with Hugh Bennett to gain a temporary advantage, I would have lost this support. During all the subsequent period most of my associates in the colleges felt they knew where I stood. No kind of compromise would have really done me any good with Eisenhower and Bennett, yet I would have lost my support in the colleges and elsewhere in the Department.

We also had some poor sessions, some of which were downright funny, including some talks about regional soil maps. And I recall a very funny paper on soil texture by M. E. Stephens - I suppose the worst one I have ever heard.

Bennett gave a banquet speech and with a little cooperation for some of his "friends" got himself pretty well drunk and alternately shouted and sobbed about how we were losing our soils.

My old friend Truog gave the address at the Agronomy Society banquet on Edmund Ruffin and soil tests. It was good and a bit funny.

After the meetings, or just before, we had a short staff meeting laying out needs for improvement of the Soil Survey Manual. Also we had many conferences about county planning in the old BAE as I recall. ~~I was also~~ All along I was having a great deal of correspondence with my former students and a few others, helping them get their graduate work under way.

insert 126a, 126b, 126c, 126d

1938. Along about this time, either in 1938 or 1939, the House Appropriations Committee was very critical of the research work of the Department on the basis that, "projects were never finished." This depended, of course, whether one talked about financial projects, each of which cover a very large area, work projects, which cover a fair percentage of a broad financial project, or research-line projects, which are narrowly defined usually in terms of one research scientist or a small group. The Secretary set up a committee consisting of bureau heads with Jardine as chairman and with me as executive secretary. Since Jardine loved to talk but didn't have the same attitude toward work, he presided at the sessions and I did most of the work with the help of an extremely able young man by the name of R. W. Maycock who worked under Mr. W. A. Jump, the distinguished budget officer of the Department. When the report was finished it was up to me with Maycock's help to sell it to the Bureau of the Budget before a group that knew nothing about research. I recall that one of them had been giving speeches

in the South about changing cropland to grass. I asked him what insects and diseases he thought grasses would have when grown widely. He didn't know. I pointed out what had happened to sweetpotatoes. While they were being grown only in gardens we didn't hear of diseases, but when they were grown in large fields two counties had to be quarantined. No one saw a headline, "Cattle in the Midwest being slaughtered at any price," because our alfalfa experts had known that the Grimm variety that was commonly grown was highly susceptible to alfalfa wilt. They had searched the primary sources and had found resistant plants. These were crossed with Grimm and other strains to give the Ranger which was resistant. When the wilt did hit this seed was all ready for large field increase.

With these and other examples I built up a case for a staff of top experts in every field of agriculture to meet emergencies; and the only way to keep such a staff in top-level trim was for them to have important research. I reminded them of the Queen and Alice running very fast and staying in the same

place. The Queen said something like this: "If your want to get somewhere else you must run at least twice as fast." In the United States we have built our agriculture up to a high level, which makes it vulnerable. It requires much technical and scientific work only to maintain the present level, to say nothing of still greater efficiency.

I asked them this question which I used many times afterwards: "If all the people of the United States - every single one - left the country as the Indians left Mesa Verde and no one came back for a hundred years, how many of our domestic varieties of plants and breeds of animals do you think would be available?" They didn't know. So I told them, "Just enough remnants to start new breeding programs all over again.

Mr. Jump, who was really one of the great men of the Department, was highly pleased with my efforts on this committee. For several years after that he always had me along to discuss

the budgeting of research to his graduate school classes. And several times he took me to discussions of a local society of budget officers that he belonged to. Yet I am sure I learned much more from Mr. Jump than I taught anyone else.

~~Dr. Matthew Drosdoff left us,~~ at my suggestion in the Autumn to work on the tung oil problem in the South. ~~He had~~ remarkable success and became a first-class investigator.

Auchter was so uncommunicative that I could not help worrying a great deal about the future of the soils work. The Yearbook was out and I began in earnest to go ahead with my book, The soils that support us. In the evenings I would work on that until I was too tired to worry. In a sense that book is a sort of monument to frustration. But I had a lot of other writing to do at the office, much of which may have been unnecessary.

1939. My routine continued and with increasing work with economists. I made progress with my book.

About January 30 I started my course on soils in the Graduate School. Near the end we had two-hour sessions to finish about May 10. A student named L. G. Porter came in after the grades had gone forward and gave me a beautiful 2-volume set of Mills' Logic, bound in leather.

The Fertilizer Review continued to publish vicious attacks on TVA and the whole idea that there could be anything to worry about in the fertilizer area. We lived in a crazy world. Dr. Knight and I were doing all we could reasonably to support TVA and get greater recognition for the urgent need of more highly concentrated phosphatic fertilizers while A. L. Mehring, of the Fertilizer Division, was writing for The Fertilizer Review and by inference pushing the low-analysis superphosphate.

In March, Dr. R. S. Smith of Illinois did a very nice thing for me. In the old days Prof. Whitney, Chief of the Bureau of Soils and Prof. C. G. Hopkins of the Illinois Station had a bitter feud. Hopkins was terribly abusive of Prof. Whitney and did his best to get him fired. This enmity had continued toward Dr. Marbut. It had started toward me.

Apparently at this point Dr. R. S. Smith told the staff: "Now look, Kellogg is a midwesterner. He is one of us. He never knew Whitney. Why not get him out here and get acquainted with him." So I had an invitation, with an honorarium, to come. I gave an address on The great soil groups and the development of social and economic institutions (unpublished) and a seminar to soil scientists and economists on scholarship improvement.

After this the staff were very cooperative with me, although they quarreled bitterly with one another for another

ten years. I recall that we bought a radio that would get foreign broadcasts with the honorarium.

About April 1, Zera Foster transferred to SCS. This was a dirty trick, leaving the soil survey report unfinished when he had got my early approval for a short period with them by promising to do it. Possibly it was good riddance. Never would I recommend him for another position when SCS was through with him.

Near the end of March three of us drove to Knoxville for the annual soil meetings in the Valley. On the way down we stopped for the night in a small hotel. During the night I became sick. I spoke at the banquet of the soil meeting and was again a bit sick plus a bad headache and backache. I went on to the University of Georgia and gave a Sigma Xi lecture on Soil and Nutrition. I gave the same lecture at Blacksburg, either on the same trip or a bit later.

After I returned home the illness of stomach and headache gradually left me. But the back staid sore. I tried one physician after another and got no firm diagnosis. The was the start of the rheumatoid arthritis in my back - spondylitis - from which I had untold misery until I learned to live with pain. It was over three years before I had a proper diagnosis and treatment except for a steel back brace first fitted in 1941 or 1942. The pain gradually increased for several years.

In April the office of Land-Use Coordination was reorganized and strengthened. We had talk of joint publication of soil surveys with SCS but there was no reasonable scheme that we could get SCS to agree to and still have the support of our college cooperators.

insert 130a, 130b, 130c,

For some time we had been concerned over the small use made by foresters in New England of our soil surveys. So a splendid conference was arranged at the Harvard Forest for June 5 to 8 plus a short filed trip. About 35 were there,

Spring 1939. Auchter talked with me one day in March about the serious problem of tung trees. Often the trees failed. They had had several soil men out to look at them but they couldn't find any trouble. He gave me their names and I didn't wonder at their failure. He asked me if I had a man who would be good at this and I told him that I did and described Dr. Matthew Drosdoff's training and experience. So arrangements were made for Matt to spend a few weeks on some of these problem areas. He settled several of their questions in the field without even needing laboratory measurements.

The horticulturists were so enthusiastic about Matt that they insisted he should be transferred to the Horticulture Division at Gainesville. I talked about this with Matt but he had no enthusiasm for it. At the moment he was hopelessly in love with soil genesis.

A few days later I invited him to lunch in the executive dining room where we had a table for two. I explained to him that I didn't know when I could raise his grade no matter how

good his work. To Auchter soil is only something in which you dig a hole to plant a tree. He could make a ten strike on this tung problem and come back to soil genesis later. He thought it over and suggested a date near the end of April, 1940 when he would have his present work completed. It was then that he left us.

He did make a ten strike and turned out to be one of the very top people in the United States at diagnosing fertility problems. (I am aware that Dr. Lyle T. Alexander is also very good.) Much later, I suppose somewhere around 1950, Dr. V. R. Gardner, former horticulturist and Station Director at Michigan State called at my office. He was retired but had taken some assignments with MSA. He had just come from Columbia and had identified nitrogen deficiency in the coffee fincas. He told me a soil man should be sent and asked me to comment on a list of names he handed me. I shook my head against all of them.

"What are you so particular about?" he demanded. "It's just a simple case of nitrogen deficiency."

I asked him, "Dr. Gardner, you've been in many more orchards than I have. Which was the last one where you found a single nutrient deficiency with no interactions with other nutrients, or with soil depth, water, light, or temperature? These men that you have on your list do not understand interactions."

"I see what you mean," he replied. "Whom do you recommend?"

"Well first," I said, "I should try to get the best man if I were you."

"And who would that be?" he asked.

I told him about Matt Drosdoff and then he recalled his work on the tung trees. "Do you suppose he would be interested and would they let him go?" he asked.

So I suggested that he go and see Dr. John R. Magnus and find out. The upshot of this was that Drosdoff did very well and liked it so well that he devoted several years from then on to technical assistance in South America and Viet Nam.

mostly from the northeast forestry schools, the Soil Survey, and the Forest Service (Research). J. K. Taylor of Australia was on tour with the Soil Survey and he was there too. No great earth-shaking decisions were reached but soil scientists and foresters got to know one another better and cooperation continued to grow and much improved research resulted. A group from this region, including Canada, have met nearly annually for years.

1939

Near the end of June Lucille and I decided to take a vacation together. We drove with the children to Michigan where they visited their grandparents. We went to Port Huron and crossed to Sarnia, in Canada, and drove directly to Montreal. We stayed mostly in private homes that accomodated tourists. One of our most interesting excursions was to the Ile d'Orleans. Until recently this community had been quite isolated from the rest of Quebec but a new bridge then made travel to the island easy. We drove northeast along the south side of the St. Lawrence with occasional side trips inland. The people were very courteous. Most of the villages were very neat and orderly. One wasn't and we asked at the next village why. The man explained to us that that village did not have a full-time priest. We noticed that early in the morning the priest would be down with the men putting out to fish. During the day he would be out with the pulpwood cutters or on the farms. Here too, we got a lot of nice pictures. We came down the east coast of New Brunswick through Massachusetts, across New York, over into Pennsylvania, and home, about July 13.

Auchter had Bradfield as a special consultant advising him, but no plan was announced. What waste and frustration! Even per diem allowances for travel were lower in the Bureau than for others in the Department.

I left Washington to join the travelling conferences organized by Undersecretary M. L. Wilson on July 20. Lucille went back to Michigan where we had left the children with their grandparents when we went to Canada.

This travelling conference was a splendid way to get acquainted with many of the finest men in the Department. M. L. and I had long visits on the train and followed the geology from the old Western Guidebooks of the USGS, while Milton Eisenhower, Dillon Meyer, and 2 or 3 others played poker in a parlor room with all the shades drawn.

We started the tour in Spokane, Washington on July 24 and looked at about every kind of work the Department had. The local men of the Department and the colleges were joining and dropping out as we went along. We visited the Grand Coulee

and central Washington. At Ephrata I saw the reclammation land classification - a combination of soil texture, structure, drainage, and relief with interpretations brought into strictly empirical combinations so that the maps cannot be reinterpreted for any other purpose than their use at the moment. Such a waste!

Then we went to Coeur d'Alene in Idaho. I was put in a big room with several others, one of whom snored the loudest I ever heard and the rest of us staid awake.

July 28 we visited the colleges at Moscow, Idaho, and Pullman, Washington, and went on to Walla Walla. And so we went day after day, through Yakima, Olympia, Chehalis, and Longview to Portland Oregon for the evening of July 27. I got some nice photos to use in The Soils that support us.

We went on to Corvallis the next day. I got left behind looking at the soil where a new labor camp had been built on a smooth, heavy soil (Dayton?) impossible to drain effectively. But I caught up with the party in a bus. We stopped at Medford

and went on past Mt. Shasta and Marysville. July 30 we saw Lake Tahoe, Mono Lake, and Yosemite, but arrived in Stockton after midnight. On July 31st we saw the big sequoia trees in the nigh mountains and came again very late to Bakersfield. Then on August 1st we looked at the San Dimos Forest Experiment Station and stopped at Ventura. The next day we drove along the coastal road to Berkeley.

August 3rd I spent some time in the morning with the Pacific Science Congress meeting in Berkeley. Beginning at 10:00 a.m. we had a big land-use conference. The Director of Extension for California (Cochoran) gave a ridiculous talk as if there had been no depression nor a New Deal. The Regional Forester and the Director of Forest Experiment Station got into a very funny debate.

The next day I spent in agricultural conferences, soil conferences, and with the Pacific Science Congress.

Sunday, August 6. I rested my sore back and sorted myself out.

The next day Macy Lapham, Ray Roberts and I started a soil tour together, looking at soil profiles, farms and vegetation. Our route took us to Salinas, Santa Barbara, and San Diego to Yuma, Arizona. August 10 we looked over the experimental station and drove into the Gila Valley. In the evening I talked long with Ray Roberts about the great need to synthesize all soil characteristics and to see the soil landscape as a whole unit (what I later called the soil individual).

August 10 we drove north of Yuma in great heat to Boulder City and, the next day to Las Vegas, which was a miserable little town in those days.

August 13 we visited an SCS survey project where G. C. Johnson and O. L. Orton were working. We looked at some excellent desert soils. The following day we drove from Beaver Dam through Las Vegas, Nevada and Mojave, California to Bakersfield. August 15 we examined many of the important soils of the central Valley and went on to Stockton. John

Retzer and I had a good talk about graduate work at Iowa State.

The next day we drove back to Berkeley and had a long talk with Charles F. Shaw. He had some absurd suggestions about how to handle soil correlation and so on. But we all kept our tempers easily.

Macy took me over to the big World's Fair on Treasure Island. Some of the pictures from Italy were fine. But the air was very cold after so many days in the hot deserts. Macy and I worked all day on the 17th making plans for assignments, for improving his quarters, and so on. That evening I left for Montana. As we were waiting for the train going north one came in from Chicago. A mail truck backed up to it but was directly on the north-bound track. Suddenly a fast train came north on it, hit the mail truck, and scattered the pieces into the crowd. Then the brakes went on. How I got back of a post I don't know. But I beat the fragments!. Happily, the men in the truck saw it in time and were in the air jumping from the truck into the mail car

when the train hit it. The train backed up, and it was my train! Of all the crazy engineers. The police came since several were hurt but after an hour or so let the same engineer go on. Believe me, no one slept until we got a new one at the next division point. We were very late, but the next engineer took this train, with its flatteded wheels, a bounding down the valley into Portland. I just made my connection to the east.

August 20 at 1:45 in the morning I got off at Malta, Montana, for the Great Plains tour with M. L. and his large group. In places the dust was terrible. We drove to Dickinson, North Dakota. I was glad to be brought up to date on the grazing association in McKenzie and Billings counties that I had been able to get going in 1934. We stopped in Hettinger, North Dakota and on to Rapid City South Dakota.

A very funny incident occurred at lunch time in Lemmon, South Dakota. Since we were too large a group for any cafe

in the town, the ladies served our lunch in the basement of a church. As we walked through a narrow hallway, I noticed some profiles of solonized soils. So I hurried through lunch and came back to look at them. The profiles included examples of the same sets I had twice published. A young SCS man said to me, "Are you interested in soils?"

"Yes, I replied." Soils could be interesting."

"Fine", he said, "I'll explain these to you!" He went through the whole business published in my paper on Solonetz soils in western North Dakota in good style.

By then others were coming out and we had to move away. He put out his hand and gave his name but said, "I didn't get your name". I told it to him and I've never seen greater embarrassment. "Interested in soils!" was all he could say. Later he wrote for a job, but we had no opportunity at that time. Too bad. We spent the evening of August 23 in Rapid City

This day I had word that Tommy Rice died suddenly in his office. What a pity!

On August 24, by starting at 6:00 in the morning, M. L. Wilson and I had a conducted tour through the Black Hills with explanations by Dr. Condra of Nebraska. We stopped in Scott's Bluff, Cheyene, Sterling (Colorado), and Lamar.

August 26 we drove through the famous Baca County, hardest hit by the 1935 drought. The Federal Government spent enough emergency money in that county during the drought to have bought all the real and personal property in it. We saw a few moving dunes there and on the way to Goodwell, Oklahoma. At Amarillo I bought some new outdoor clothing. We went on to Clovis, New Mexico, and staid the night of August 30 in Las Vegas. The following day we reached Albuquerque and went to Durango, Colorado. This day, September 1, the Second World War began in Europe. Everyone felt that we would become involved.

The following day at Mesa Verde was extremely interesting. The Indians left after a long period of drought and left their cliff palaces and all. They had terraces and check dams from

On this trip I had occasion to drive a good deal with Mr. Paul Roberts of the Forest Service. He was in charge of the Prairie States shelterbelt program and had worked closely with Frank Hayes and the men I had sent to assist him. Paul was one of the finest men I ever met in the Department and had a much broader view of agriculture than did most foresters. Although I shall have to say that, on the whole, the leaders of the Forest Service have shown up better than those of any other Department agency.

the early 15th century. M. L. guided us to many Indian villages. After Gallup we went to Flagstaff near the Grand Canyon. On the way we stopped at an Indian village and watched a rain dance. No one knows how old it was. But the squaws wore bright colors, contributed by the Spanish. In the wind-swept sandstone of the main street I saw a beautiful set of dinosaur tracks not far from a parked new car! And four days later it did rain, very hard.

We had conferences of the group, swelled with visitors, at Flagstaff for three days. Many gave brief speeches including me.

I was to leave for Chicago early September 7. About midnight I was awakened and told that a storm had washed out a stretch of the railway line and the last train for several days was soon to leave. So I hastily dressed and boarded the train. We left at 1 o'clock the morning of September 7. All the next day we took turns with M. L. He was in great pain, apparently from bursitis. We got into Chicago about

8:50 the next evening. I got a room and went on to Grand Rapids the following day. Lucille met me there and we drove back to Ionia September 9. On the 11th we started for Washington and reached our home the evening of September 12.

I had a very busy week to be able to go out again. SCS had still another "Soil Conservation Survey" handbook (USDA Msc. Pub. 352) out with the "land-use capability" classes. It was hard to justify and still harder to follow.

I left Washington again on September 20 and went with another travelling conference but this time without M. L. We started at Detroit and went to Midland, St. Johns, and Lansing. There we had an evening conference.

We drove from Lansing through Ionia, Belding, and White Cloud to Ludington. We crossed Lake Michigan to Manitowoc, Wisconsin and drove to Fond du Lac. We staid there the night of September 22 - what little there was left of it! The following day we were at Madison and stopped for the night at La Crosse. September 24 we drove to Morris,

We had lined up enough lectureships, including the Soil Science Meeting, to support the full travel of Prof. G. W. Robinson in the United States. I had worked hard on this for several months. Since I had to be in the west in August, Dr. Alexander met him in New York. They had only been our a few days when the Second World War started in Europe. He went back to Wales at once and never got any of the fees. I felt terrible sorry to have missed him. And I know his personal budget must have been tight for a long time.

Minnesota and the next day stopped for the night at Bemidji. We drove near Cass Lake, Grand Rapids, Floodwood, and Cloquet. We got to our beds in Duluth about midnight. The following day we drove to St. Paul by way of Iron River, Hayward, and Barron, Wisconsin. We had a fine meeting at Barron. The last day was a big land-use conference at University Farm. I left St. Paul one minute before midnight September 28 and got home again September 30.

On this trip and after I broached the idea of a monthly get together for dinner and talk at the Cosmos Club. I do not recall exactly the starters but Kimball Young, Bushrod Allin, Gove Hambidge, Jack Fleming, Max McCall, Warren Thornthwaite, Ernie Wiecking, and I were included. We had our first meeting early in November and continued once a month on Monday evenings until November 1942. Some dropped out and others came in. Sometimes we brought a speaker but more often a member lead the discussion. If the speaker faltered the boys took over. No one was quoted but commonly

the going was a bit rough.

Much had to be done to prepare my speech for New Orleans, get on with the book, and take care of the staff. So I did not have much time to worry about either Bennett or Auchter. Yet I did prepare long memos for Auchter on the new SCS survey handbook, land-use capability, and other aspects of the work.

Sometime in late October 1939, I was asked by the Editor of Land Policy Review to review a book called Vanishing Lands, which was an American edition of The Rape of the Earth, written by G. V. Jacks and R. O. Whyte. I glanced over the book and sent it back saying it was a poor book and not worth reading. Then Mr. Jack Fleming of the Bureau of Agricultural Economics came to me with the book and said, "I should like very much to have you review this book. Secretary Wallace has been reading it and is very much taken by it. Probably he is being misled." I replied, "He certainly is if he is taking it seriously." And I agreed to review it. I finished the review about November 3, and sent along extra copies, one of which was sent immediately to Secretary Wallace. He had already written his speech for the land-grant meetings and had it mimeographed. These mimeographed copies had some quotes out of the book but when he spoke he omitted all reference to it.

I had some very interesting fan mail as a result of this review. Some of them suggested that I should give some of Hugh Bennett's books the same treatment. But Mr. Jacks was quite put out by it and has never been very friendly since. He almost never reviewed anything of mine in "Soils and Fertilizers unless it just couldn't be helped. Years later

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Jacks wrote another little book and asked me to furnish him some photographs, which I did. He thanked me for these photographs and told me he hoped I did not get to review his new book. But he sent me a copy and it was harmless enough. I never could figure out why Jacks wrote such a terrible book as this The Rape of the Earth except to make some money. He was always complaining that he did not have enough money.

I wrote another little book and asked me to translate him
[illegible] papyrus, which I did. He thanked me for these
papyrus and told me he hoped I did not get to revise
[illegible] book. But he sent me a copy and it was humiliate-
[illegible]. I never [illegible] out why Jesus wrote such a
[illegible] le book, as this [illegible] or but Jesus except to make
[illegible] ry. He was always complaining that he did not have

In the middle of November I was invited to appear before the Land-Grant Association meeting in Washington and present a paper on Policy and objectives of the Soil Survey, which they published.

We arranged a way to pool expenses of key staff to the meeting of the Soil Science Society of America in New Orleans. We started a staff conference of the Soil Survey beginning November 20, two days in advance of the Society meeting. Present were McKericher, Moon, Roberts, Hearn, Nikiforoff, Lapham, Thorp, Hayes, Carter, and Baldwin. We discussed current problems of soil classification and mapping still unclear in the Manual and Soils and Men, of which there were many.

The funniest exchange was between Moon and Hayes, who brought strength to the staff. We were discussing the recognition, definition, and mapping of phases of eroded soils. Much of the talk was about a reference horizon within the soil. Obviously, one could not map erosion;

it is a process. Nor could one measure what is gone, except by reference to some standard. Finally Hayes said, "I don't see why all the trouble Joe (speaking to Moon). We have a perfectly good reference within the soil - the top of the lime zone". There was a pause. Moon looked at him and smiled. Suddenly Frank said, "Oh, I guess that wouldn't work everywhere." And we all had a great laugh.

During the Society meeting Bennett was reported to have made a tearful speech for cooperation to save the soil while there was time but ended by advising his listeners to get the biggest club they could find and hit anyone over the head who criticized the program (of SCS). Probably that was intended mainly for me, who was attending a different section.

I gave a main session paper on Soil classification and cartography in relation to other soil research. (How I have had to hammer away on that theme!)

Generally, I seemed to have the support of the State people, although I had to be very vague indeed about plans in the Bureau of Plant Industry. I hated to admit that we had none. Many did know that the new building for the Plant, Soil, and Nutrition laboratory on the Cornell campus was getting under way.

Dr. W. H. Pierre was elected President for next year and I Vice-President, much to my surprise. This was the only way the Society had of showing its support.

(Sometime later Bradfield told me that he emphasized this to Auchter - but Auchter was not to be moved. He possibly blamed me in part for the dispute with SCS but I had no alternative but to see Bennett get all the soil research and ruin it.

But basically he was jealous of me. He thought my interests were too broad. Economics was a closed book to Auchter and he was afraid of economists. Later Dr. McCall

told me an illustrative story of his attitude. He happened to be in Auchter's office when the USDA Yearbook, Food and Life, came out. As he thumbed through it he came upon an article, The nutrition of fur~~bearing~~ animals, by Charles E. Kellogg. McCall reported that Auchter exploded, "This is the last straw! What in hell does he know about fur animals?" McCall explained that the author was a different man by the same name - a rabbit specialist in the Bureau of Animal Industry.

During the meeting I had asked certain ones to report on the quality of the mapping by the SCS field men. The replies suggested that the boys tried but had too little supervision. None of them had yet fully sensed the great danger of the uncontrolled legend except Moon, Hayes, and Ableiter.

During December I worked hard on the book. At the end of the year I took a trip to Nebraska and stopped at the University of Chicago and I believe at Iowa State also.

At the beginning of the year I was in Lincoln taking part in one of the BAE "schools of philosophy" called "Public Affairs Institute". I much enjoyed Walter Prescott Webb who talked on the same program. I don't recall my exact subject but it dealt with the importance of strong local leadership to maintain our democratic traditions. I also talked to two other groups arranged for by Hayes and Condra. I was home again about January 7.

< insert 151a

This was a busy time indeed. Much effort was spent on the committee dealing with county planning and I was working nights on the book. Again TVA was trying hard to get me to leave the USDA and come with them. Again Dr. Knight advised me to go. But I felt I could not let so many people down who were trying so hard, both in the Soil Survey and in many of the colleges.

We started a series of seminars on soil structure that ran from January through April. Both Nikiforoff and Thorp were trying only to discredit one another. The sessions had their funny side but they demonstrated clearly that

M. L. Wilson didn't recover from his bursitis as fast as we had hoped. With 1940 an election year he didn't think he sould continue as Undersecretary without being able to carry his fair share of the campaigning. So he resigned and became Director of the Extension Service.

neither of them was capable of objective study and research. They argued not for the truth but only to win.

In early February I began two courses in the graduate school, one in soils and a seminar on land-use planning. I had to miss the odd one but we finished in May.

Move to #1

~~In February, I gave a public lecture at the University of Michigan.~~ ~~and went on to Iowa State College.~~ The troubles with SCS continued, of course, and we had much trouble to get an assistant for Frank Hayes under the existing committee system.

~~March 3 and 4 I received another honorarium for speeches and seminars at Iowa State. My speech was Great soil groups and cultural patterns. And I had seminars with both the soil science and the economics staffs. Visits to Iowa in those days were great morale builders for me, partly as a respite from the terrible pressure at my office.~~

Move to #1

The midwest group interested in soil survey planned a meeting in Chicago for March 8. They asked me to send an inspector, but did not invite Baldwin nor me. We sent Frank Hayes.

I am sure they did not invite E. A. Norton, but he invited himself. He gave away his secret about going the day before. He found an excuse to come to my office in the afternoon, fishing to see what I was doing the next day by asking something about one of my classes. It was obvious what he was fishing for. Then he left abruptly, saying, "I must get going because I am taking the train for Atlanta." So I told Baldwin that Norton was going to Chicago.

Hayes sent me a report of the meeting and told me later that he never saw a man get such a blistering lecture as Norton did. They did not like him, what he stood for, or his horning in on the meeting.

In March and April I prepared long and detailed memoranda to Auchter about the technical points at issue with SCS. I do not know how much good ~~they~~ did beyond the self discipline of setting down the facts precisely.

Warren Thornthwaite was having trouble in the SCS, mainly,

I think because he was smart. Yet he did have a peculiar personality: One day he would be gay and the next he would be sour and not even speak. He could and did write some of the best scientific papers; and he could and did write some absurd stuff. He had had good bulletins turned down because "Kellogg would never let them by." He had courage enough to bring them to me and I wrote memos of favorable criticism. So they got published.

Thornthwaite wanted to bring Hugh Bennett and me together. Bennett called me one day and suggested that he and I go together and look at the geomorphology research around Spartansburg, South Carolina. He said, "I think we should talk to each other and not get so much second hand." I agreed with this sentiment and promised to go. But he got cold feet and did not go. I did.

We spent four interesting days in the field from April 1 through 3. A fair-sized group of soil scientists, geologists and ecologists were there. These men had good

data to show that much of the gently undulating Cecil soil, which is all supposed to be from saprolite, is actually developed in local alluvium. At one time the whole area had been deeply gullied; then the gullies filled; and, about the year 1000 A.D. normal gullying began in a new cycle. In new deep gullies, peaty material was found 15 to 30 feet deep where the new gullies had cut deeply into the old ones that had been filled. Above the deep organic material, were gley horizons. (This might be why Bennett did not come. He worked in this region over a period of many years and missed the geology badly.)

A bit later I made a short visit to both Knoxville and Blacksburg between classes. (All classes were double hours.)

About this time I had a few highly critical letters about the SCS work and cooperation from state college people on surveys that I sent on to Bradfield. Old M. F. Miller, whom we all loved, had no fight left in him.

On top of all our other problems, I had some appropriation cuts - defense cuts that made things a bit tight. But most others did also.

In May I had my book essentially finished except for my final editing and typing. So I wrote to Schoenmann asking his permission to dedicate it to him. (Schoenmann had left TVA, gone to the University of Michigan for a short time, and returned to Lansing because his wife was unhappy anywhere else.) I had recalled that he told me as a student, "People will soon forget me, but you will be writing books on soils".

"If I do," I replied, "I shall dedicate the first one to you".

Sometime in this general period - perhaps a year or so earlier - after it had been agreed that all inspectors would be mutually approved, I got a call from a personnel clerk in the Department Personnel Office about a small point in a job description for an inspector. I asked him whose it was. He

replied - a Mr. W. A. McLenden. I told him I would be over in a few minutes. Then I called Dr. Barnes and we met there. The young man was a bit scared but we assured him that all he had to do was return the sheet to SCS and ask to have Bureau clearance as provided in the Secretary's memorandum that set up the arrangement for joint inspectors.

I never heard another word about it. McLenden was a sort of spy for Bennett. He had been in the old Bureau of Soils and was fired. I recall the story I heard of the day that he came to see me while I was away. The door of my office was open and he drawled to my secretary, Mrs. Preiss: "Mickey, how about goin' in and have a nap on Dr. Kellogg's davenport?" She replied, "Good heavens, Mac, since when have you had to lie down to go to sleep." No reply was reported.

In June and July, we were pushing to complete the soil survey of Barron County, ~~Wisccsin~~ Wisconsin that both the Dean at Wisconsin and I had promised the local people the previous September.

My morale was very low in June but I tried not to let it show to the staff. They had expected the Soil Survey to be swallowed up by Bennett and they had to be protected all I could.

I recall that it was this summer that Bennett proposed to put a huge request in his budget for "completing the land inventory of the United States in ten years." Eisenhower called me and told me the Secretary had asked him to make a judgment about it. "What would you do in my place," he asked.

I replied that if I had the problem I should invite the heads of the agencies concerned - Bureau of Plant Industry, SCS, Forest Service, Office of Experiment Stations, Extension, and the Bureau of Agricultural Economics - and let Bennett present his scheme. Then the group could discuss it and vote. He promised to do just that.

The chips were really down. I talked a bit with each of the men who would be coming - all were friends of mine, because I had tried to help them.

I recall that the day was very hot. I rested my sore back a bit before the conference.

Bennett brought Norton with him who turned the big charts on an easel that had been prepared for budget hearings. I can recall only the first one - a picture of a small girl drinking a glass of milk. He talked for an hour and then answered questions. Then Eisenhower turned to me and said, "Tell us your views".

I described the process of soil classification and mapping and the relationship of the soil survey to other research. Basically its purpose was to serve as the bridge between experience and the results of research, on the one hand, and specific tracts of land on the other. I emphasized the essential need of a standard, continental nomenclature for this process to work. From the soil survey one could find the alternative systems of use and their physical inputs and outputs. At this point the soil scientist joined hands with the economist to arrive at recommendations. I also explained the need for

close cooperation with the state colleges.

I don't believe I took more than 20 minutes. Very few questions were asked. Then Eisenhower said to the group, "Gentlemen, there you have it. I am going to ask each of you to vote." I got all the votes except Bennett's. Eisenhower said to Bennett, "the decision is clear. You will not present this to the Bureau of the Budget."

So far as I can recall, Bennett did not speak to me again for over 10 years, and mighty little then.

About July 1 we moved to an apartment at 109 George Mason Drive ~~for~~ from the house on 26th Street south in Arlington. We had accumulated some furniture and bought a good deal more. This was a three-story apartment with a full basement.

~~Later I went down to the Valley and with Mr. Moon went to south Georgia.~~ and 100a

This year, as vice-president of the Soil Science Society I had to arrange for the program, which required a lot of time.

About the second week in July I went down to the Valley and Mr. Moon and I drove down to Tifton, Georgia for a conference on grassland agriculture. I had worked up a good speech for this on Potentialities for grassland in the south. I remember talking with Gove Hambidge about this and he wanted it for a proceedings of the conference. But somehow the plan for the proceedings got held up for a year or so and actually never was done. On the way back we drove by Joe's farm in south Georgia and then visited the farm of Harry Brown, former Assistant Secretary of Agriculture, in north Georgia. The next day we went back to Knoxville and I came home.

About the middle of August, Dr. John D. Black and I worked together in developing a program for TVA of analyzing the records on the test-demonstration farms. For some reason McAmis was not really keen about this urgent need. We spent most of the time in the field looking at farms and their systems, and at the records. We worked on the report for several days and suggested some reorganizations. This all took me until August 20.

After that I worked to complete the book. Mrs. Preis worked on it in evenings for which I paid her. And I hired Eddie Grimes to make the finished drawings. 161a et seq

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September 7 I left Washington for Iowa State College and had conferences at Ames along with trips to the experimental fields with special emphasis on pasture grass. This gave Pierre and me a chance to go over the Society program. I was back home on the 13th of September.

Dr. Pierre was busy on the land-grant committee on the

Since Henry Wallace was running for the vice presidency with Roosevelt he resigned as Secretary of Agriculture. It was years afterward that I realized what a great secretary he had been. This doesn't mean that he was without fault. He wasn't the best judge of men in the world and made some mistakes on administrative appointments. He was a great student of comparative religion. The mystical and bizarre often appealed to him as well as the scientific and philosophical. On more than 2 or 3 occasions some mystical quack would sweep him off his feet by interview or book. He would see his error but sometimes a bit late.

Wallace was much stimulated by anything intellectual. He was well informed in a large number of fields. Most men learned to stick to their own speciality on talking to the Secretary. In any other field he was likely to know more than his interviewer. He was always looking for ideas and especially unconventional ones. Any time that the telephone rang it could be the Secretary. (I don't mean he called me

frequently but still he often did about all sorts of things. Sometimes he had a question. Other times he would give me a bit of information that he thought I might have missed. I suppose for 10 years I would get the odd query or note from him. And so did a lot of other people.) He was much interested in the relationship between soils and the political ideas that the people developed.

M. L. Wilson also had a wide-ranging curiosity. While he was Under Secretary he arranged for a good many lecturers to come to the Department - priests, archeologists, co-op leaders, philosophers, and aoo sorts of other people. Commonly each one would give a lecture in the auditorium in the late afternoon. Then the next morning some 20 to 40 of us would crowd into M. L.'s big office and have a free-for-all seminar with the lecturer. Often the Secretary would come in. Then both the Secretary and M. L. would organize all sorts of other ad hoc little or big seminars just to kick around ideas. If the boys didn't get to arguing by themselves, Wallace would

prod them until they did. I never knew just how Wallace and M. L. selected the participants for these seminars but certainly it wasn't done by age, grade or administrative position. In fact, as I recall, Howard Tolley was about the only bureau chief who usually came. But everyone knew that the Secretary was interested in ideas and no other bureau chief would dare to interfere with a man in his bureau taking part.

This intellectual atmosphere was as good as any I have ever found in any university. In fact, any university could be proud to have such kinds of tough, far-ranging seminars.

When Wallace left and Wickard came in the Secretary's office ceased to be involved. Yet M. L. sponsored a good many seminars in extension and so did Howard Tolley in BAE. Up until the end of the Second World War we had a whole series of committees on post-war planning that Howard Tolley promoted. But a combination of bureaucratic administrators in the Department and Farm Bureau leaders stirred up the Congress to make the Department cut out the county planning. Finally

they got Tolley too.

Most of the members of the old seminars left the Department during the War or immediately after it and they weren't replaced by like men.

Gradually, this kind of intellectual activity among people of different agencies and disciplines declined. It hit complete rock bottom during the administration of Benson. Some of us hoped that it would revive with the coming of the New Frontier. Most appointments were much better. Yet the Secretary was unable to establish anything like the intellectual communication with the staff as a whole that Wallace had.

Soil Survey for the mid west. He developed a good report which ultimately had a lot of influence.

In October Auchter and I went to the Valley and out in the field with some of the mappers. He had a good chance to talk about the work of TVA, including the soil survey, with Moon, McAmis, and others. Then I went over to Columbia, Missouri for the meeting on land classification. I gave a paper there which was also published in the Journal of Farm Economics: Contributions of soil science and agronomy to rural land classification. Ableiter did very well on soil survey interpretations. A. B. Lewis of Cornell flunked badly and soon after that his silly scheme was ended. The University of Missouri published the proceedings as a bulletin. It soom became a collector's item.

Much had to be done in October for the Society meetings, including a speech that Auchter was to give at the banquet.

I got Hambidge and Fleming to read the manuscript for my book and Auchter gave approval for outside publication on

November 26 and I sent it by express to Macmillan the next day.

At the Land-Grant meeting Bradfield was formally elected chairman of their soil survey committee. I hoped then they would get rolling.

We had a Soil Survey staff conference in Chicago for a few days in advance of the Society meetings, which ran from December 4 through 6. McKericher and I drove out. The roads were terribly slippery. Our car had no heater so I stopped in Indianapolis and had one put in. What a drive!

At the staff conference were McKericher, Thorp, Lapham, Hayes, Moon, Ableiter, Nikiforoff, Youngs, Fitzpatrick, Williams, Carter, and Baldwin (?).

One evening some of us decided to go to a night club for dinner. I'll never forget the look on Hayes' face when he saw the prices on the menu. He had a bad evening, while Moon loved every minute of it.

We were now making good progress in developing the long-term plans for the committee work essential to the revised

Soil Survey Manual (1951).

This was a very busy Society meeting since, as vice-president, I was in charge of the program and, as the incoming president, had to get committees lined up for the next year. I was especially anxious to have a good committee on the improvement of curricula in soil science. We wanted young men and older men; people from industry, government, and the colleges; and geographic representation. As chairman I chose Prof. E. Truog, who did very well indeed.

After the meeting, McKericher rode back to Washington with someone else and I drove to Ionia County. I staid with my father and mother a couple of days and brought Lucille's parents back with me for Christmas.

While I was in Chicago a check came from the Country Gentleman for $275 for my piece on Are our soils ready for a war? (March, 1941). I also completed most of the manuscript for a three-part paper on The scientist and social policy in the democratic state. (Sci. Monthly: May-July. 1942.),

based on a speech that I gave at the AAAS meeting in Philadelphia - Agricultural science and the democratic state - after Christmas.

1941. Although I never did get on a good working basis with E. C. Auchter (and I was one of an enormous company), we never did have any open break and I learned how to deal with him. He had an unreasonable fear of being laughed at. Once I learned this I kept the secret to myself but always had this in my mind when I wrote him any kind of recommendation.

January 21 I went up to Ithaca, New York primarily for the first collaboration meeting of the new Plant, Soil, and Nutrition Laboratory. I went a bit early for conferences at Cornell with Frosty Hill, Bradfield, and some of the soil survey men on the basic problems of soil groupings and land classification. By an informal arrangement with Bradfield, I gave, with him, both Gordon Johnsgard and Marlin Cline a long oral examination on their theses. Cline was essentially letter perfect. In the evening I heard a lecture by Prof. Hoagland of California on his research.

The meeting at the new laboratory began the next day, January 23. Auchter had all three of his degrees from Cornell

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~~In February Macmillan accepted The soils that support us for publication.~~

and looked a great deal to Cornell for men to staff the lab. He had selected Dr. Maynard for Head. Except for Kenneth Beeson, who had started the work with me, and one clerk, all the other appointments were agents - a clear violation of Civil Service rules. (It took a long time to get this business straightened out later.)

Maynard had been well briefed and gave all the credit for the ideas to Wallace and E. C. Auchter.

The seminar went forward with discussion about the (1) crop approach, (2) animal approach, (3) soil approach, and (4) survey approach. Of course Maynard had not thought about this problem before and misses the whole point of interactions. The new plant physiologist had a great deal to learn about soils, light. water, and their interactions. I agreed to prepare a memorandum about the principles of geographic synthesis.

~~In February Macmillan accepted The soils that support us for publication.~~

The whole general field was explored and I think probably the conference was helpful. I could see that it would be very difficult to run this laboratory because each group of specialists would want research in their fields and would tend to go along the same lines as the hundreds of other research groups in soil science, plant nutrition, and animal nutrition without ever taking advantage of the unique opportunity of the new laboratory to work in the border areas. It took quite awhile for them to establish cooperation with the other experiment stations. But some good work was done later.

Now I was having a great deal of correspondence with Dr. Black about the work in TVA, the land-use seminar, Gettysburg, and many other common interests.

This year I started my two classes again, one in general soils and the land-use seminar. We started about the 6th of February and ended near the end of May.

In February, I gave a public lecture at the University of Michigan and went on to Iowa State College. March 3 and 4

I received another honorarium for the speech and seminars at Iowa State. My speech was Great soil groups and cultural patterns. And I had seminars with both the soil science and the economic staffs. Visits to Iowa in those days were great morale builders for me, partly as a respite from the terrible pressure at my office.

In February Macmillan accepted the soils that support us for publication

From March 22 to April 1 I went to Knoxville, first of all for the meetings about soils primarily for those working in the Tennessee Valley on a cooperative program. These were about the most valuable conferences in soils I had ever attended. The enthusiasm was remarkable. For one thing these meetings were organized as an integral part of the work, not something apart from it. The boys were trying to find out how to do the collective job in the most effective way, which was how to use science to help farmers arrive at the best possible decisions about the use of their resources. They not only had to know about soils, crops, water, and livestock but also the goals and spirit of the people.

I gave the usual kind of speech stressing the inevitability of war and of the kinds of adjustments agriculture would be faced with. I talked with them about the place of the Soil Survey for synthesizing all of the data that bear on the productivity of the soils under alternative systems of management.

I tried to steer them away from extremism, including specialization that shuts our other knowledge, the shallow generalization, the "burning" liberalism, and so on.

Dr. Black also came down to this meeting and I introduced him. I told the boys to expect some criticism but that it wouldn't be too rough because he likes us and the work too much. Schoenmann was there too and we had a great deal of good talk about how to get things done. We spent a little time in the field and I worked pretty hard trying to get a long statement on nutrition prepared to the satisfaction of Dr. H. A. Morgan.

In April I worked on the index for the book. I had quite a little rowing with Macmillan at this point and later. I had supposed, of course, that they would have an editor go over the manuscript. But they hadn't and I had to get an editor to go over the galleys. Then they wanted to charge me $100 for the changes. But I talked them out of the charge since they had saved more than that by not having an editor themselves.

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April 1941. I don't remember the day exactly but I remember very well the incident. Norton had been trying to get Barnes and me to agree to a proposed committee rule that no representative of any of the cooperating agencies could visit a soil survey without first having the approval of the inspector. I pointed out that we had been trying hard to encourage the state experiment stations to carry on supplemental research. Such a rule would work against that and so it was turned down.

A little later Dr. William Pierre wrote to me that he had a letter from A. M. O'Neal saying that this was the rule indicating that Dr. Simonson couldn't go out and collect samples in the county without prior permission from him. I replied to Dr. Pierre that there was no such rule and that it was hard for me to believe that O'Neal would write such a letter. So by return mail he sent me O'Neals's original letter.

So I called O'Neal and told him that I should like to see him for a moment when he had the chance and he came over. I told him in a kindly way that Dr. Pierre had gotten the idea

that he, O'Neal, was promulgating a rule that Simonson and other staff members couldn't go to cooperative soil surveys without his approval.

O'Neal said, "I don't see how he could ever have gotten that idea from me."

I asked him, "You didn't write such a letter to Dr. Pierre?"

"Oh, no." he replied.

So I pulled the letter Bill had sent to me out of the drawer, handed it to O'Neal, and asked him, "Is your signature on this letter a forgery?"

O'Neal never answered. He turned white and then red and stumbled out of the office. Dr. Baldwin told me later that he came in to see him and complained that I had insulted him.

Then, of course, along with my other work I had the usual fussing with SCS about every conceivable point as standard operating procedure.

On May 13 we got the sad news that Frank Hayes had died suddenly. About 18 months before this I had insisted that Moon should spend some time with him in his region and that he should go with Moon to the Valley. By all accounts Hayes, Moon, Roy Simonson, and A. P. Nelson had a wonderful trip together in 1940. Hayes was to have left for the return trip with Moon in the Tennessee Valley on May 14, the day after he died.

I had corresponded with Dr. Condra about the possibility of Thorp in the place of Hayes. On June 26 I had a conference about this whole matter and the SCS problem. Dr. Condra had the rather crazy idea that he could bring Hugh Bennett around and convince him that I should come into the SCS on my own terms. In fact later, he sold this to Milton Eisenhower. I don't recall the date but Milton insisted that we go to lunch

together and he made this proposition. (He had, in fact, made similar ones before.) According to Milton, I would go into SCS in charge of all scientific work, including the Soil Survey and all other research on my own terms without any interference from Bennett. I knew that Milton Eisenhower couldn't deliver on this and that I would lose all the friends I had in the process. I tried to tell Milton that people with my training simply could not sacrifice the basic principles they lived by. And that I knew he wouldn't be able to understand this. It is often the truth that hurts. Milton walked with me back to my office telling me that he too had basic principles. But he didn't, except for Milton to get ahead. He was a pure opportunist. We were never intimate again.

Of course, before I left Lincoln, I had to make a speech following one of Dr. Condra's impossible introductions.

In June, Mary Alice went to Michigan by train and Robert went to Boy Scout camp. Lucille and I took a week's holiday

in New York City. We went to Macmillans and prowled the old book stores, saw a show or two and bought a few pieces of furniture that came down later by truck.

← insert 174a

In August we were happy to see The soils that support us in print despite 2 or 3 irritating little mistakes. (It seems to me that the first copy of every book or large bulletin that I have ever written always opens immediately to a page with a printer's error.) This little book did much better than I had expected, or Macmillan either. After each of the first few printings they would let it stay out of print a few months; but it had nine printings and the 9th one was of pretty good size. It was still selling in 1973 and 1975. And the USAFI (or "foxhole") edition in paperback ran about 90,000 copies in addition to the trade copies which must have run at least 15,000 or more. I was very unwise not to have stored away some first editions. Although I had been a book collector for quite a while it didn't occur to me to save my own book! After the

One day in the summer of 1941, as I recall, several Chinese came into my office from the Chinese Embassy and offered me a nice stipend to go to China for one or two years to organize an agricultural extension service. I protested that I had never worked directly in their field. They smiled and said they had looked very carefully into my record. It would have been an interesting assignment and I could have learned about a part of the world of which I knew very little.

Later I took the matter up with E. C. Auchter and he was much opposed. He said, "I couldn't possibly be responsible for granting you leave at this critical time to go to China and possibly not getting back. Let them get Lowdermilk or someone like that."

I explained that Lowdermilk was no kind of organizer. But Auchter was firm and Lucille was far less than enthusiastic. So I turned the offer down. (~~Again~~ As it turned out, my guardian angel had her arm around my shoulder.)

first edition, which was printed on nice quality paper with nice margins at $3.50, the war restrictions came on and all subsequent editions had poorer paper, smaller margins and higher prices.

Robert came home from camp and he and Lucille drove to Ionia as I left for Knoxville again August 10. After some preparation we went over for a look and for talk about a whole watershed demonstration near the Tennessee-North Carolina line. All farmers agreed to use lime, phosphate, and to improve their pastures and other crops.

I recall talking with one farmer in his kitchen during a brief shower. As I looked over his records I could not see that he had made any additional income. So I asked him, "Your records show no increase in net income. How come you are so enthusiastic for this program?"

"Just look at my land," he exclaimed.

I could see that a great fear had lifted. He had sensed that his soils and his farm were deteriorating. He might need

to give up but he had no other place or skills. Now he knew he could make it.

After the meeting we drove back to Knoxville for more office work and later drove over toward Asheville for a look at the experimental watershed of the Forest Service - Bent Creek. As I recall it was on this trip that we spent a day in the field with Dr. Salter from North Carolina. I returned to Knoxville for 3 or 4 more days with Moon, McAmis, and others with TVA.

Shortly after I had returned home sometime in September, Salter joined the staff at the Bureau of Plant Industry to be in charge of the Division of Soil Management, which was made up of odds and ends from scattered projects in the plant divisions, the old Division of Soil Chemistry and Physics, and the old Division of Fertilizers.

Sometime before this Auchter had brought in O. C. Magistad to be Assistant Chief of the Bureau for Soils. So far as I knew he didn't do a thing except call me over one time to talk about the Soil Survey, which we did for about an hour. Salter told me afterwards

that he had asked Auchter what his relationship should be to Magistad. He said Auchter replied, "Just do like Kellogg does; he ignores him." Not long after that Magistad left very bitter against Auchter. But this made him only one of a large company. Magistad had previously been Director of the United States Salinity Laboratory at Riverside for a short time. None of us ever knew why Auchter brought him to Washington nor what kind of instructions he gave him.

During October Dr. Bradfield and his committee completed their report on the soil survey problem for the Land-Grant Association. He gave me a chance to review it in considerable detail and it was presented to the Association at their meeting in November. They recommended that the basic soil survey should be handled in one research agency and that as a first step all inspection and correlation should be centralized. They had several other recommendations along this same line and drew attention to the inadequacy of the so-called "land-use capability classification". We couldn't have asked for anything better.

The Society meetings were in Washington during November 12 to 14 inclusive and I did my stint as President. We also had a conference of the senior staff of the Soil Survey during the few days beforehand.

My morale was a bit higher than it had been, with the book out and the full support of the state people on my position vis a vis the SCS.

Then on December 7 came the catastrophe that we all expected - the entry of the United States into the Second World War. If the stupid Japanese had followed through their attack, they could have easily taken Hawaii and held it for quite some time. If they had not attacked at all, the Allies might not have had the military support of the United States until quite a bit later.

By memorandum issued December 15, 1941, the Secretary grouped the research bureaus under an Agricultural Research Administrator and appointed Dr. E. C. Auchter, administrator. Later, February 23, 1942, the Secretary issued an executive

order creating the Agricultural Research Administration and defining its functions. For some time Auchter took on this job and remained Chief of the Bureau of Plant Industry.

I took another short trip down to Knoxville just before Christmas, mainly on advisory work with TVA officials but also to confer with Mr. Moon.

At Christmas time I made a deal with Lucille and the children that if they wanted to come to New York with me while I went to the annual meeting of the Association of American Geographers I would pay their travel and hotel and all the meals they ate with me. So we drove up during the day of December 27 and stayed at the Barbizon Plaza hotel. We spent some time sightseeing and we saw Aida at the Metropolitan.

Much of the program of the geographers dealt with land economics and the post-war planning. We also had an excellent section on geopolitics which sent me to read McKinder, Mahan, and Spykman. Dr. John D. Black was there for part of the time and we had an excellent joint session with the political scientists.

Lucille and I went down with the crowd at Times Square while the old year went out and the new one came in. We decided that this was a fine thing to do once.

1942. We all drove back home on New Year's Day. I think everyone had a nice time in his own way.

During the last year the arthritis in my back got steadily worse and more painful, especially after relaxation. This would be relieved in the middle of the night by a few moments of exercise. I went to several different physicians and got different answers - some of them outlandish. At the recommendation of my clinic I went several months to an osteopath who turned out to be a bit of a quack. At the recommendation of friends I went to a Dr. Engh. I think he was a good man but things got no better.

Also about this time another problem loomed. We were to move next autumn from the South Building to Plant Industry Station north of College Park. Many of us lived in Virginia and would need to move. Further we would have to pay all of our own utilities and this would be equivalent to a substantial reduction in appropriations. But this was Auchter's pet baby and he was bound to do it.

This came about in a curious way. Before coming into the Department, Auchter was at the University of Maryland and had a house nearby. He came in as Head of the Division of Horticulture and persuaded the Department to build a couple of buildings for horticulture north of College Park which would be near his home. Then when the defense program started the military wanted the land in Virginia on which most of the Bureau's research was located. Auchter made a deal with them that if they would build the buildings he wanted for the whole Bureau out in the same place he had chosen for the horticulture buildings, and would acquire the necessary additional land, he would give them the land in Virginia. Thus the funds for thses buildings and the land were in the military budget and the agricultural appropriation committees of the Congress never knew about it until the buildings were built.

The post-war planning committees, generally headed by BAE, were by now quite active. For awhile I headed one on industrial decentralization, but after the later move to the Plant Industry Station I simply had to give it up although I

took as active a part as I could in several of them. In fact, when I look over the notes of these committees I don't quite see how I managed.

With 2 or 3 others I attended a conference on post-war planning in Carbondale, Illinois on January 13 and 14. This old coal area was in a ~~bad~~ bad way economically. We had an interesting conference with the local leaders. They started out telling us how very badly they needed additional dams for water storage. At the end of the meeting, however, one of the local leaders asked our patience to discuss an unrelated problem. They were pushing for a military training field. They gave all of the advantages of the Carbondale area over alternative sites. When the speaker came to water he confined himself to the single statement: "The Carbondale area has abundant water of excellent quality." Not a one of these Chamber of Commerce members sensed the inconsistency. On the return trip from Carbondale to Chicago we had a conference with some of the people of the railroad and state and local officials.

I took this trip with Roy Kimmel. He was hard to travel with. Just about train time in the station he had to call "Aunt Mary". Then when he got on the train he discovered he hadn't any money and he needed to borrow. When we got about half way through a trip he suggested a different itinerary.

(To be inserted in early 1942.)

What started out sometime in late 1941 to be a vicious attack by Hugh Bennett actually seemed to turn to my advantage in some ways.

Although E. A. Norton was nominally in charge of the variously called Physical Land Surveys and/or Soil Conservation Surveys, Bennett used him as a hatchet man on all sorts of unsavory assignments for years. I didn't learn of the beginning of this particular effort until after the whole affair was about over with. It started out with either Bennett or Norton, or both of them, explaining to Secretary Wickard and his powerful assistant, Carl Hamilton, that, basically, their difficulty on cooperation with the Soil Survey was due to the fact that Kellogg simply didn't know anything about soils. Secretary Wickard didn't know me to amount to anything, nor had I met Carl Hamilton at that point.

Secretary Wickard directed Carl Hamilton to make an investigation of this charge. Since he was a graduate of Iowa State, he started by interviewing Dean Buchanan, who was director of the agricultural experiment station and dean of the graduate school. By chance, his secretary was engaged to marry one of my former students. While the dean had this conference

with Hamilton, the door to his office was not closed, and she heard the discussion and reported it later to her fiance, and he to me. According to her account, the old man simply blew up and told Carl Hamilton that the trouble was exactly the reverse--that Kellogg had a fine reputation throughout the country, and that no scientist had any confidence in either Hugh Bennett nor E. A. Norton. After this considerable lecture, the dean is reported to have said, "You don't have to take our word for this. You can go to any of the land-grant colleges with strong departments of soil science."

Hamilton said, "Well, what are some of those with a good staff in this area?"

The dean mentioned Wisconsin and Cornell. Apparently, Hamilton went to these two, and perhaps others, and reported to the Secretary.

Then again, Norton repeated the same charge in the Secretary's office. This time he was told, "You get back to your office and stay there. If you come over here once more, you'll have no office to get back to!"

Sometime in early 1942, as I recall, I was called over to advise Secretary Wichard on some technical point that I have forgotten. When I came into the outer office, I walked up to

Carl Hamilton's desk and said, "My name is Kellogg, and I am scheduled to see Secretary Wickard in a few minutes."

This was the first time we had met. Hamilton leaned back in his chair, put his feet on his desk, and said, "So you're Kellogg. I have wanted very much to meet you."

"The way you say that," I replied, "You must have expected a man with a gun in each hand and a knife in the other, and maybe horns."

"At least that much," he said. "I think I have heard you called every name in the book from 'abysmally ignorant' to 'one of the top soil scientists in the United States.' At least, a number of people find you interesting!"

By then it was time to talk with the Secretary, and I never heard any more about this unhappy incident.

On March 10 I started a considerable trip beginning with a conference in Ames with both Dr. Pierre and his staff and Dr. Schultz and his staff. I then went on to Berkeley, California. It was cold and there was snow in Sacramento. Macy Lapham was ready with a government car. He, Ray Roberts, and I drove over to Salinas. Here Major Kelly and Paul Roberts had their headquarters for the guayule rubber project. They were developing nurseries like mad and were just starting to scout the areas for leasing land for production. I had loaned them Mr. Frank O. Youngs to head up this work. I remember asking Paul Roberts how he was doing. Paul said, "Doc, if a man around here even looks as if he knows what he is doing I never question him or bother him." John Retzer and Clint Mogen were also working with Youngs.

We staid about 2 days and then drove on to Mojave and through Baker to Prescott, Arizona where I looked over a soil survey being rather poorly handled by Bartholemew. He had taken Dr. Marbut literally when he shouldn't have and was ignoring the geology. (Marbut tried so hard to establish soil science as

distinct from geology that some interpreted him as feeling that geology was unimportant to soils.) The soils here were new and interesting to me. We visited Skull Valley and drove down to Casa Grande and from thence to Brawley in California.

The Soil Survey, the University, and the Bureau of Reclamation had a joint soil survey of the East Side Mesa in California. A fellow named Ralph Cole who was a graduate student with me at Michigan State was in charge. This was a ridiculous place to consider irrigation. Most of the soils were very sandy and any disturbance of the vegetation would start serious soil blowing. In fact, we drove through a serious sand storm while I was in the area west of Yuma. The movie people were filming a Sahara love story but they surely didn't look romantic that day. We went to Yuma by way of Calexico and visited the experimental station at Yuma.

March 25 I left Yuma at 3:45 in the morning and got to Houston, Texas at 10:00 the next evening and then I went up to College Station, Texas for conferences with Connor, Carter Fitzpatrick, and others.

On March 30 I left Dallas by train and went to Knoxville for conferences with Moon, McAmis, Bass, and others about our soil survey work and the project that Dr. Black and I were promoting for the economic analysis of test-demonstration farms. On the train to Washington the trees were covered with ice and beautiful. But many were broken.

When I returned to the office in early April it appeared that the House Committee would cut the budget for Soil Survey. In this I could see Hugh Bennett's fine hand. But, of course, I hadn't been able to appear myself and the budget presentations were poor. (That year we did end up with a cut. Of course, I cried about it but it did give me a chance to get rid of some people most of whom transferred to the SCS the next July.) At that point Dr. Baldwin made a comment that got back to Hugh Bennett and Hugh really went after him for it for the next few years. All Mark said was: " These transfers raised the average of both organizations." The trouble was that his comment was true.

I had anticipated our entry into the War and had kept good tires on my car. As a result it was stolen April 21.

The police found it out in the country with the tires off and with considerable damage from breaking into the trunk. The car was insured but we couldn't get anything but old recapped tires during the War. Later the thieves were caught and severely punished but the tires had been sold in the black market.

April 24th I went up to Harvard University and ~~had~~ ~~conferences with~~ met with Dr. Black's seminar and had conferences with several of his associates. While at Harvard I saw Orvedal who had been accepted there for a year with Dr. Black on a nice fellowship. (Unhappily, he was arbitrarily drafted into the army just about a week or 10 days before the end of the year. Dr. Black was very high in his praise of Orvedal's study.

A few days later I attended the meetings of the Earth Science Division of the National Research Coundil. These are about the driest meetings that I know about. But later the soils work was moved to the Division of Biology and Agriculture and most of those meetings were still drier.

Despite the appropriation trouble things began to look up because it seemed that the Secretary was going to order the consolidation of the soil correlation work in Soil Survey.

Just after the middle of May I went to Wisconsin to give a lecture in the "Public Affairs Institute" run by the BAE. The local people had complete control of who was invited to speak and what they should speak on. I gave a lecture on the geopolitics on war and natural recources - The strength and weakness of our natural resources in total war (unpublished) - and explained what would happen if the Germans took Africa and could cross to Argentina. I gave the lecture first at Wausau. In the afternoon I got my first look at a top-notch, large commercial silver-fox farm. The food and sanitation probably exceeded that of my hotel.

Then I was driven over to Eau Claire and gave the same speech. By prearrangement I left Eau Claire about 10:00 p.m., arrived in Madison at 3:00 a.m., and Truog and I had a nice visit in his home until my train left at 8:00 a.m. Truog was worth

~~Truog was worth~~ all the trouble but I am not sure that I was.

I had lots of work to do in planning how to take care of the budget cut and being sure that the transfer memorandum was signed.

June 4th I left in the evening for Ithaca and attended the collaborator's meeting of the Plant, Soil, and Nutrition Laboratory. This meeting was better than the last one. For the first time I met R. Q. Parks, an old student of Salter. Later Salter brought him down to his Soil Management Division.

After the collaborator's meeting I spent a day with Dr. Bradfield and his staff and got back to Washington on a Sunday.

On the 11th of June several of us had a conference on rural industries in the State Department. I don't know that I helped get many industries into rural areas but the work on the committee did a great deal for my education.

In June for the first time I was fitted with a steel back brace, which I had to wear from then on.

Secretary Wickard's memorandum number 1020 dated June 23, 1942 was issued. This transferred from the SCS to the Soil Survey all the people concerned with soil correlation, including salaries, travel funds, and equipment. Yet the funds were carried in the SCS appropriation for the 2 or 3 years following. The effective date of the transfer was to be about July 6, as I recall, when Hugh Bennett, Auchter, and myself were all scheduled to be in Mexico City. So another order was issued for a postponment until August 1. This order was in line with the Land-Grant report of Dr. Bradfield's committee and relieved a lot of tension.

On July 1 several of us left Washington for Mexico City. Lucille went along and we had a pleasant trip, especially after Professor Schultz joined us at St. Louis.

We arrived in Mexico City, July 5 and the Conference began the next day. There was nothing particularly earth shaking at these conferences but there was a great deal of good will which the United States badly needed with the war ahead.

The sessions were staggered enough so that we had several opportunities to look at the countryside and farms near the city and the usual sightseeing at the Aztec pyramids and nearby cities, including Cuernavaca. Along this line I was most impressed with the Church of Guadaloupe, dedicated to the Brown Virgin.

By far the best soil work was that being done by the Irrigation Commission. We went out to the agricultural college but I wasn't much impressed, except with the beautiful anti-American, pro-communist murals by Diego Rivera in the chapel.

I had a prepared paper on Soil classification as a basis for a sound agriculture, published in 1943.

Hugh Bennett even carried his feud against the Soil Survey into this conference. The Latin Americans were impressed with the Soil Survey Manual and one of them introduced a resolution that the Conference officially recommend this to all governments as a basic guide for soil survey work. Bennett raised a curious objection: "This Manual is not the work of

one man. Many people have contributed to it." One of the professors pointed out, "Of course an author draws on the work of his predecessors. This is acknowledged and documented in the Manual." This kind of talk kept up for a long time. The chairman asked me if the recommendation would embarrass me. I told him that it wouldn't at all, but that of course I couldn't support the recommendation personally. The chairman took a vote. I abstained. Bennett voted no. And everybody else voted for it, including the other Americans. The Latin Americans were shocked at this sort of thing. But I was becoming used to it.

Anyway my back was hurting so much here that I didn't much care. Possibly it was the high elevation. At night I would have terrible cramps in my back muscles. Fortunately I had gotten the name of a reliable Mexican physician. Each morning I went to his office at 8:00 and he gave me a shot of novocain in the back. This kept me going. The Minister of Agriculture felt sorry for me and gave me a nice inlaid cane.

Yet everything considered we had a good time and left the evening of July 16. Lucille got off in Austin, Texas on the way back to visit Mabel Lindsey (later Mabel Schmidt). And I went back to Washington. As I got warmer and into lower elevation the back pains eased a bit.

Shortly after returning, the President of Cornell University invited me to give the Messenger Lectures there in 1944-45.

During 1942 Milton Eisenhower left the Department to work with Elmer Davis on war information. But he didn't stay there very long before going to Kansas State as President. Reliable people told me that he took the presidency with the promise that the Republican leadership would put him in the Senate. If so they welched on the promise and he left Kansas after 7 years for Penn State. Other reliable people deny he had any such promise. Anyway he made a better record at Penn State than he had at Kansas.

I went right ot work on the plans for the consolidation and reorganization of the soil correlation work. We planned to have four regions, which was fairly easy to do. I planned to have Moon in charge in the south, and Youngs in the far west. I offered the one in the Great Plains to Roy Hockensmith, who turned it down on the basis that he could help the program more where he was. (But he couldn't.) Had he accepted I should have brought Thorp to Washington for the northern region. Later on I tried to get Guy Smith for the

northern region but he had gone into the army just a few days before my letter came.

As the first of August approached I discovered that Hugh Bennett had been able to get another postponement until Sept 1 of the successfully ignored the Secretary's order. There was a bit of confusion in the Department over the ~~responsibilities~~ relations of the Secretary ~~in relation~~ to emergency war production problems, ~~and~~ SCS was grouped with some of the other action programs. USDA, in fact, had two "secretaries."

Yet with pushing, the order was signed by W. A. Jump, E. C. Auchter, M. C. Townsend, and Secretary Wickard August 31, 1942, and the Department Budget Office worked out the details. The transfer finally became effective September 1, 1942 because of the great help of Mr. Black.

Along about this time Gove Hambidge told me about some animal research in Britain that he had been reading about which showed that vitamin E deficiency brought on muscle spasm and suggested that I try it. My physician had never heard of it but we worked out a mild dosage and it did stop the terrible muscular cramps. Subsequently I stopped taking it 3 or 4 times

and on each occasion the muscular cramps returned. After that I didn't experminet anymore but kept on taking small doses.

August 7 I went by train to East Lansing for conferences about soils at Michigan State. we reviewed the whole cooperative program. Then I went on to Ionia County for a few days leave and Lucille, Mary Alice and I came back together by train from Detroit.

In September Schoenmann was appointed collaborator to advise on the Soil Survey program. Now the special ire of Bennett and Norton was directed against Mark Baldwin and I had to hold his little warm hand. Baldwin was already angry because Norton had accused Baldwin to several people, of illicit sexual relations with one of the secretaries on the SCS payroll who had been helping Baldwin. She had promptly resigned. (After this order I was able to get her back.) I had had quite a time keeping Baldwin quiet. He had said to me,"I'm going over and punch Norton in the nose."

I told him, "You are not going to do any such thing. Just because he gets down and wallows in the gutter you don't have to follow him. No one believes his tale -- no one at all. But if you get in a fight with him people may think there may be something to it."

I had expected Hugh Bennett to demand some sort of investigation about how the men were using their time. So for each I fixed up a few standard categories and required each inspector and assistant inspector to report weekly how he spent his time. These elaborate records were kept for several years.

Auchter called all of us in one day to announce that Vince Cardon would become Assistant Administrator for Research. He also announced that Dr. Salter would become Chief, Bureau of Plant Industry with Frank P. Cullinan as Assistant Chief. This was quite a shock to a lot of people, especially since he had not discussed it with Dr. M. A. McCall who was certainly a much better man than Cullinan. I am sure that Dr. McCall was more hurt by this exhibition of lack of confidence than he was by not being made Assistant Chief.

I grew to like Cullinan quite well, personally. He was a good plant physiologist, but he never became any kind of an administrator. He was one of the few that was entirely loyal to Auchter and reported to Auchter all the gossip that he heard. For a long time Salter was Chief of the Bureau in name only. Auchter still made the appointments of division heads and gave many of them direct orders. And Salter could find out about it the best way he could. Really no man should have wanted any job as badly as Salter wanted that one.

I had determined that now was the time to get my top inspectors into P-6. I had drawn up elaborate charts in three colors showing the administrative lines, scientific guidance lines, and cooperative relations to all state and federal soil scientists. Almost to my amazement this went through without any hitch.

Move to Plant Industry Station

We had moved to the Plant Industry Station in September, 1942, and it was a terrible nuisance to drive from Virginia. So we had started looking for a house and made arrangements to buy one at 4100 Nicholson Street in Hyattsville. We moved to the new house on October 31, 1942. I had helped pack the truck in the morning and then Mary Alice and I drove on ahead with the car full of fragile personal items. When I stepped into the house the only piece of furniture was a telephone, and it was ringing. This was a call from Paul Roberts in Salinas, California asking me to furnish him some 20 or more additional soil scientists. I told him, Paul, I don't have that many I can spare."

He said, "I know you don't, but you know the soil scientists in the country. You figure out how much each of them is worth, offer each a job at that salary, and wire me the names, salary, when each can come, where he is, and I'll carry on from there.

I would have taken these risks with mighty few people besides Paul Roberts. Ultimately we had 30 men on this project without a hitch in a single appointment. I had to take Frank Youngs off to be regional inspector for the western states and we put John Retzer in charge of the soils work on the guayule project. At Paul's insistence he got a double jump to P-5.

In the autumn Auchter appointed Frank W. Parker to fill the position left open by Dr. Salter's promotion. At least Salter gave me to understand later that this wasn't his appointment.

After some time in this position Auchter made Parker Assistant Chief of the Bureau for Soils. This seemed to many of us as a very strange appointment. I talked with Salter about it and he said that he had nothing to do with it. If at that moment I had had one of the nice offers that I had had before of had after that I should have left. Fortunately I didn't. — insert 200a through 200d

After Parker's call, I threw my feet up on the desk and said to myself, "Those feet are going to stay there until someone has the guts to come along and push them off. From here on I am going to work on the things that interest me with not more than half of my time on administration." And that's the way it was.

Shortly after this appointment Parker came over to my office for a talk. He said that he hadn't asked for the job but seemed to hint I might be a "controversial" person. He was quite condescending and even said that people like me were needed to stir the others up and keep them on their toes. I let him understand that this was far from my principal role, especially now with all the conflicting points of view among soil scientists and fertilizer interests. I was trying my best to play a judicious role and the going was difficult. Certainly I knew that Bennett disliked me but this could not have been avoided by any means short of agreeing to have all of the soil research, including the Soil Survey, going to SCS. Had I compromised I should have lost all the friends I had. Of course, I might lose some of them anyway if the going got to be easier.

Parker turned out to be a rather complex man. He had been thoroughly indoctrinated at Dupont and always took the same line as Charles Brand on the fertilizer question. He had

kept up some of his earlier connections on the War Production Board and used these against TVA. (This was confirmed in a curious way. One Sunday Lucille and I had been asked to Parkers for dinner. Frank had been called away or something so we had dinner with his wife and two daughters. The elder one remarked, "Daddy is going to be sorry, just after he left a letter came from the National Fertilizer Association. He always jumps when he gets one of those.")

Except possibly for Auchter I never knew a man so confident of his own poor judgment as Parker. He could direct a staff as a foreman but he didn't know how to use one as an administrator. With his mind made up on a proposition he would say just about anything to push the idea or to get support. Later I spoke about this once to Salter but he didn't think Parker would lie. So I informed myself on three cooperative relationships Parker had worked up with other people in the Bureau. I interviewed them and confirmed the facts. Then on three

separate occasions I made a casual telephone call to Parker and asked him a question in such a way that it would be slightly to his advantage to lie. And he did all three times. But Frank didn't actually think he was lying. He was just doing the obvious to win a point.

On the other hand Frank could be and often was well mannered, affable, courteous, and generous.

I didn't know it then, but I was bound to have trouble with him so I did have my guard up most of the time. For one thing, he tried to get hold of Soil Survey funds. (This was some time later). Both he and Salter had used some of the funds of the old Division of Soil Chemistry and Physics, originally for Soil Survey laboratory work for other purposes. So Dr. Alexander was short of money. As needed I hired some soil chemists on our payroll and assigned them to Alexander. The Soil Survey paid for some equipment, travel, and supplies. Parker spoke to me about a transfer of the funds to the Division

of Soil Management. I wouldn't agree. I called in all of my administrative people and senior staff and explained they were never to agree to this. Then when I would be out of town Parker would work on Ableiter. Ken's defense always was the he couldn't understand. (One day Frank said, "I don't see why you think Ableiter is such a good man. He seems a bit stupid to me." He was just doing his duty.) Parker urged Salter to have me do this and Salter tried it. I told him that I couldn't sign such an order but that he was Chief of the Bureau and he could. I said, "If you do I shall file a memorandum with you predicting that the funds will be misused, as were the funds formerly used for laboratory work that I now have to make up by employing people on our payroll." Salter got red in the face and said, "All right, we will leave it as it is."

In November I had an unpleasant incident that hung on quite awhile. In the reduction in force of the previous July, the first man to go was the poorest one - E. H. Bailey. He hired a lawyer and wrote up a brief against me that was ridiculous. Auchter was very sensitive about such things and insisted that I take him back since we now had ample funds. I told Auchter, "If that man comes back here to work you get my resignation." At least at that time he didn't want it. Bailey kept bothering congressmen and others but all we had to do was insist that they have a conference with him.

A lot of work had to be done in our new home. For one thing I had to get plywood and make blackout shutters that would fit all the windows or we couldn't have a fire in the fireplace. We got these made and had some practice drills just before I went to St. Louis for the annual meeting of the Society and a brief staff conference with my senior staff who were there. Baldwin was ill, I also spent a bit of time rounding up some fixtures for the house.

Salter was ~~there~~ at the SSA and had been pushing me very hard to

appoint the fourth principal correlator. Baldwin had suggested earlier Earl Fowler who was working under Moon. After I had failed to get Dr. Guy D. Smith I didn't know anyone better than Fowler. Salter and I had a conference with Moon about it. Moon said, "Doc, every time I see you, you either take a man away from me or give me more responsibility. This process has got to stop."

After Moon got this off his chest I said, "Well now that you have said that, what about it?"

Moon replied, "I'll agree to it if you will let me name his successor without any strings tied to it."

I said, "That's all right, Joe, if it's legal. I wont promise to do anything outside the regulations." I turned to Salter and he said, "That goes for me too."

Moon said, "All right, I want Dr. Roy Simonson. He is one of the brightest young men in the business." So that is how Simonson came into the work.

As soon as I returned we initiated the correspondence and he was appointed senior soil correlator at Knoxville, March 17, 1943.

Later in November I got involved with Professor James and other geographers in the development of the Office of Strategic Services.

- insert 203a and 203b

I finished the year with quite a bit of writing, including a lecture portion, and considerable work with the post-war planning committees. About this time , I'm not sure just when, Sherman Johnson and I began the study of the fertilizer consumption that would be expected in the United States in 1950 but at least we were developing our ideas and working out a plan.

Transfer to p 226

During December I took a bit of leave plus night and Sunday time to complete one of the upstairs bedrooms. It always takes a lot of carpenter work to adjust a house to a particular family's needs.

Along about this time, a bit later or a bit earlier, I was propositioned to go to the Middle East with headquarters at Cairo for a new outfit that Vice President Wallace was heading called Economic Warfare. The Germans had been doing business with the Arabs, - buying their rugs and so on - and the United States wanted to outbid them by offering them small tractors and other goods. We had long talks about this and the troubles that Lawrence had had on a similar mission in the First World War. I didn't want to get caught like he had with a lot of promises he couldn't deliver on. Finally I agreed to go provided I could get the answers to two questions: (1) A clear statement of United States policy vis a vis the Soviets in Iran and (2) What would be the firm minimum shipping space I could count on. My interviewers said these were reasonable questions and they would let me know. Two or three weeks later I met one of the men on the street by chance. He told me that he hadn't been able to get the answers to either question but he would let me

know. It ran along then another month or so and a young man called on me in my office and then we talked in Appleby's office about this position, which he had accepted. Later he came around to see me at home and gave me a book. I never saw him again but I am sure he hadn't asked the same questions. Poor fellow!

1943. Shortly after the first of the year Salter brought by my home the notification of my grade promotion from P-6 to P-7 - the one that Auchter had blocked in the autumn of 1938.

This winter my back continued to get worse despite the brace. The several physicians could not agree on the cause. They thought it could be some kind of arthritis but they were not sure. It seemed essential that I get a proper diagnosis. The physicians agreed that the Mayo Clinic would be the best so I made an appointment there for June 2.

During January I had further calls from the military for strategic maps and photographs. I had already given them a great many - two of great importance they said. In 1938 I had bought examples of various kinds of maps in France. Among others was a new, highly detailed, military map of the Mediterranean coast east of Marseilles. Our intelligience had been so poor that the officers told me this was the only one of these maps available in the United States. (It covered the area where General Patch's army landed to go up the Rhone Valley.) Then

I also had the nice panorama of the Naples Harbor I took in 1938.

The exact date escapes me but along about this time a large group of officers came in and asked me about the color of Desert soils. We discussed it and I told them that some deserts were reddish, some gray, and some yellow. Then I said, "You gentlemen are interested in some particular desert and you don't know whether you can trust me or not. I shall leave the room and tell the girls not to pick up the phone. You talk it over and do any calling you need to. I'll return in about 15 minutes." When I got back they told me the exact places they were interested in along the north coast of Africa. Fortunately we had a few soil samples from there and some maps. On the planned invasion the Army wanted to take along only the essential materials for camouflage.

Baldwin was still ill and didn't get back to work, as I recall, until late summer. He had periods when he was quite irrational.

In early February I made one of my usual visits to Knoxville in order to arrange the soil survey program for the

rest of the year and to discuss TVA problems with the staff. During many of these visits I would attend a Board meeting but unhappily I didn't record which ones.

By now the post-war planning committees were going strong. I sat in on several of them, which was quite a nuisance with my office at Plant Industry Station.

The Soil Survey was being called on for more and more work on strategic crops. The guayule rubber project of the Forest Service on which we were cooperating heavily was perhaps the most important. But other kinds of rubber-bearing plants were also investigated, including a dandelion-like Russian plant called koksaghyz. This never came to much in competition with guayule. We also did some work on milkweed and especially goldenrod. Fiber flax was in short supply so we did quite extensive sork on finding where it could be grown successfully. We also did extensive work on locating soils in several states suitable for castor beans.

One of our most dramatic successes was in setting the county production goals for peanuts. Farmers were offered a premium price but the Department first set the goals on a historical base, which didn't work at all. Some counties had nearly all of the soil suitable for peanuts already in peanuts, whereas other counties with abundant soil had almost none. It all depended on how badly the boll weevil had been on the cotton. With the old published soil maps and new reconnaissance maps we were able to set realistic goals. And the peanuts simply rolled out of the ground. Curiously, the additional supply was consumed largely as roasted peanuts, rather than as peanut oil as we in the Department had expected.

We also did some exploratory work with the Madagascar rubber vine in Mexico and with both Hevea and Castilla rubber in South and Central America. Ray Roberts took one of the toughest jobs in the swamps of Central America to find suitable soils for emergency production of abaca - or manila hemp - used in marine cordage.

Some work was done to get grasses started quickly on military airfields and how to keep the grass from catching fire.

Besides these projects we had a great many quickie ones to help develop allotments for chemicals and machines. I recall a curious one. Because of the shortage of steel, spring-tooth harrows had a low priority compared to disc harrows; yet farmers working stony soils couldn't use the disc harrows. I had some 48 hours in which to list the counties where spring-tooth harrows would be necessary, along with an estimate of the percent of cropland involved. Of course, we could use only published soil maps.

By far our most important work, as it turned out, was the development of terrain-intelligience maps of the going conditions for tanks and self-propelled canon. Unbelievable as it may sound, our assault forces in the Pacific had no terrain-intelligience except for the beaches. This accounted for the disaster at Guadalcanal. Early in the War we teamed up with the Geological Survey and made such maps. Few soil

maps were available for any of the critical areas. All file and library sources were searched for air photos and especially for maps and descriptions of relief, geology, land form, vegetation, and climate. Since every kind of soil results from a specific combination of these factors, highly trained and experienced soil scientists could make reliable estimated maps, which were then interpreted in terms of their engineering properties as can be done with any reliable soils map. The Pacific high command was extremely grateful and asserted that this work saved the lives of many thousands of American soldiers. After that time this work became one of the most important research activities of the Soil Survey, and indeed of the Department of Agriculture, with some field work and with world-wide operations. Many of the improvements in our published soil surveys, including engineering interpretations and good legends and design for general soil maps came out of this work, headed for years by A. C. Orvedal.

Both before and during the War the Soil Survey was heavily involved in making highly detailed soil maps for land appraisal in the Tennessee Valley. It seemed a pity to cover with water these soils for which such beautiful maps had been made.

The pre-Spring came a bit early. I looked over our lot and decided we could have a "victory garden". The whole family got a great deal of exercise out of this. Just a short while after Robert and I had spaded a considerable area a cloudburst came. Fortunately it came on a Sunday when I was home. The water rushed from the hills above and started to cut rills. We went out in the rain with stakes and boards to stop it, and later built proper earth terraces.

Later I experimented with many plants and methods, which resulted in a nice garden, dozens of garden lectures, and a book on gardening.

In March we loaned Nikiforoff to the Office of Strategic Services for the duration of the War.

On March 26 the Food Production and Distribution Administration, which was soon called the War Food Administration, was established by Executive order with Judge Marvin Jones as Administrator. This made things very difficult and gave us in effect two Secretaries of Agriculture. Wickard had been a good Secretary. But to get food production really going he made some decisions that politicians, including the leaders of the Farm Bureau, did not like. His staff warned him. But he asked them only, "What is the right thing to do?" Most of the research activities remained with Wickard. The main part of the War Food Administration was the AAA and related activities. Curiously, the SCS now claimed to be a "food" agency and was also included with the WFA. And this spelled trouble.

This Spring I spent a good deal of time on post-war planning. Sherman Johnson and I were ~~getting going~~ working on ~~the~~ fertilizer estimates of fertilizer demand and production under the same set of economic assumptions as all the other pasture estimates. About the same group also had the job of planning the program for the Hot Springs Conference of

the United Nations for developing proposals for an international agency and program in the field of food and agriculture. I was asked to be one of the United States participants but the Conference came at exactly the same time I had scheduled for the Mayo Clinic and I didn't think I should postpone that.

In parts of April and May Lucille was in Michigan with her critically ill mother. Mrs. Mark Baldwin stayed with us during the week. Mary Alice, Robert, and I looked after ourselves over the weekends. It was very curious about Mary Alice: When Lucille was here she took no responsibilities at all and yet when her mother was gone she did quite well. We not only put out a highly successful victory garden, but also started with azaleas. After the war these squeezed out the victory garden and quite a bit of the grass.

After the War Food Administration was formed I began to hear remors that SCS might refuse to allocate their share of funds to ~~the~~ soil correlation. Nevertheless in late May I visited Wisconsin for conferences with Truog and Muckenhirn. I took leave June 1 and went to the Mayo Clinic. One isn't there

more than an hour before he becomes simply a number. I registered and had X-rays. Then the next day I had a whole series of blood and other tests. By June 5 the physicians were ready for counseling and urged that I go to St. Mary's Hospital where I registered the next day.

The hospital routine was broken only with tests, X-ray treatments, and the usual heat and massage. About every organ that I knew about and some I didn't got a good look-see.

June 17 I was transferred to Warrell Hospital for a tonsilectomy. My tonsils were removed by a poor surgeon at a cheap rate when I was a boy about 11 or 12. The job had been poorly done and the physicians felt that the remaining old stubs might be infected. (One was a bit.) The surgeon was extraordinarily expert but he told me that my throat would be sore for a long time because he had to cut through so much scar tissue. (What an understatement!)

On June 20 I was taken back to St. Mary's Hospital for more therapy and a typhoid vaccine that raised my temperature very high.

On June 24 I paid my considerable bill and went home with a brief stop-over in Madison. For several days I could eat only small pieces of food washed down with warm water.

The clinic had given me a detailed design for a special baker. Dr. Frank Parker had one made for me in the shops at Plant Industry Station. I used it for years, along with massage by Lucille in accordance with the clinic's ~~xxxxx~~ directions and corrective exercise. The disease had been diagnosed as rheumatoid arthritis of the back or spondilitis. They gave me no hope of recovery but that I could learn to live with it. The back would tend to freeze and partly the heat and massage would help keep it straight as it froze. For a long time after that I also used about 3 aspirins every 3 hours and exercised a bit in the middle of the night to reduce the pain.

A book I read at the Clinic had a good description of the pain: Put your thumb in a vice and tighten it as much as you can stand. Then give the vice another one-half turn!

Perhaps the most important thing I learned at the Mayo Clinic besides the diagnosis and the benefit of the heater, came from reading a book in the medical library - The psychology of arthritics. Most arthritics become mean and irritable. The reason is that since people know they are in great pain they try to do things for them. But other people have their own lives to live and often forget. Then the arthritic is inclined to blow his top. So the advice to arthritics was: "Do your own work, pick up your own pencils, and do your own chores."

Indirectly I was helped by President Roosevelt. When I would start to feel sorry for myself I was reminded of all he had to do and that his condition was worse than mine. He could not even walk by himself.

Sometime earlier the Land-Grant Committee on soil survey had suggested that there be a joint committee between the Department and the Land-Grant Association. This got under way early in the year with Pierre, Baver, and Bradfield representing the state colleges and Salter, Bennett, and M. L. Wilson

representing the Department. They (or their representatives) held a meeting in Washington in August 1943, partly because SCS was attempting to withdraw their share of the funds. But the Committee didn't get very far with Hugh Bennett so the problem finally fell back on me again.

Bennett had made the curious argument that this use of funds for soil correlation would weaken the potential of the SCS in war food production! I found out that his memorandum was lying on the desk of John B. Hutson, an old farm management friend of Dr. John D. Black. So I called Dr. Black at his home in Belmont, near Cambridge and explained the situation. He asked, "Charles, what can I do about it?"

I told him, "John, I want you to come to Washington. For obvious reasons I can't issue you an authorization but I'll make it up to you in someway. I want you to find a reason to go in and see your old friend Hutson. Then while you're there I want you to say, "And by the way..... You can take it from there. You know about the consolidation of the soil

correlation in the Soil Survey. It should be continued as the Land-Grant Association has recommended."

Dr. Black did just that. Afterwards he told me that when he called the matter to Hutson's attention, Hutson asked him, "Do you know anything about that? I've got Bennett's protest lying right here on my desk."

Dr. Black responded,"It just happens that I know a lot about that. The colleges have worked on this for a long time and feel very strongly about it. If you succeed in your program you must have their full cooperation. but you wont if you don't continue this consolidation of the soil survey inspection work and the funds for it.

Hutson and Auchter agreed on a committee to study the question and make a report to the War Food Administrator. This committee was V. P. Cardon, R. W. Maycock, and Sherman Johnson (who was then in the WFA on a special assignment). They recommended and Huston sent forward a statement to the war Food Administrator that endorsed the Land-Grant Committee approach. So that crisis was over and buttoned up about November 2, 1943.

While this was going on I had many other things to do including a special post-war planning session in Milwaukee (so everybody could "get away from it all") July 26-31.

Then in August I spent a few days with Bradfield on the correlation problem and I also looked over the work on the Mt. Pleasant research farm and in the Plant, Soil, and Nutrition Laboratory.

During this time especially the Soil Survey people and the state people were having a good deal of trouble with the SCS surveys, now called "utilitarian surveys". It was the same old story of uncontrolled legends with thousands of units grouped into a few wide-ranging ones. Most of the supervision from the regional and state offices of SCS, except for the small cadre of inspectors and assistant inspectors, was hopelessly inadequate.

We did have one good piece of luck in ~~September~~ September - Dr. Dachnowski-Stokes retired from the payroll. Perhaps it was

my fault, but I had been unable to get anything useful out of him to assist the Soil Survey.

Then I had another piece of very good luck - Dr. Iver J. Nygard joined the staff, presumably as assistant inspector to Mr. Fowler. He had many excellent personal qualifications, a wonderful sense of humor, and great industry. But with the women folk he had a way of saying something wrong at critical occasions. And the more he tried to fix them the worse they got. The SCS boys had told him that he should never change to the Soil Survey because of me. We became good friends and worked together a great deal. Yet I did spend many a weary hour translating his Swedish syntax - "Iverish" we called it - into publishable English.

Along in October, I believe, the Science Committee of the American-Soviet Friendship Society wrote to Secretary Wickard about a special program to be held November 7 for commemorating the 10th anniversary of the recognition by the

United States of the Soviet Union. They listed in the letter distinguished American scientists who had agreed to speak. The Committee knew that agriculture was very important but they didn't feel competent to select a speaker. They asked him to do so and I got the assignment with no request on my part. So I prepared a paper dealing mainly with the great Russian school of soil science begun by Dokuchaev, the founder of modern soil science about 1870.

So on November 7 I took the early morning train to New York. On arrival I went directly upstairs to the New Yorker hotel without even going out on the street. I met the Committee and made my speech along with 4 or 5 distinguished people. I recall Dunbar of Yale, S. A. Waksman from New Jersey, the explorer Sir Hubert Wilkins, and Harold C. Ury, from Chicago, Nobel Prize winner for Chemistry in 1934. The audience was like any university audience with no politics whatever. Just before the conclusion of the seminar, the chairman announced some new honorary members of the Science Committee including

each of the speakers.

I prepared a report of the meeting and asked what I should do about this. Dr. Cardon checked it with various individuals in the ~~Department who thought~~ Department who thought I should take part at Department expense. One advised him to check with the State Department, which he did. Two or three weeks later he called me and said a man from State had called and said they didn't think much of this organization and advised that I "do nothing". He refused to put anything in a memo. I had the file in front of me when Cardon called and scrawled across the top of it "Cardon: do nothing." Years later I got in trouble over making this speech at the order of the Secretary but without that scrawl it could have been worse. I didn't go to meetings nor answer correspondence except a few times about specific books suggested for translation or something like that.

Years later, beginning I believe in 1947 or 1948 we had to fill out some kind of a form against subversion on which

I pointed out that my name had been carried on this Committee but that I had never taken part. This initiated a full-scale investigation. Many of my North Dakota students were interviewed and hundreds of old neighbors and associates. Even at least one trap was laid. I recall that a man came into my office, I believe in 1948, saying that he had been sent to me as an expert on Soviet farms. I explained that I had been on only three - one state farm near Moscow, a collective farm near Moscow, and an individual farm north of Yakutsk. I soon formed the impression that he was an FBI man from his shoes, hat, and briefcase. Then he asked me where the collective system would be best adapted in the United States. I stepped to my map case and pointed to a part of eastern Montana where a large farm having irrigated land, dry framing, and ranch land would be economically more stable than a farm having only one. "Of course," I said, "this can be done by an American individual with the money or by a corporation. If you mean the adaptability of the political and bureaucratic aspects of the collective, I can say that it

doesn't at all fit the genius of our people and I can't imagine that it would work here anywhere."

During all of this explanation my ~~guest~~ visitor became increasingly restless. Finally he blurted out, "Well I can see you're approaching this from a scientific point of view." I told him that was all that I would be able to do and he left.

Before this interview I had been to the Soviet Union but the investigators weren't much concerned with that. I gave them copies of my Soviet Journal and my file on the meeting I had been directed to attend in New York City. Some of them got quite friendly and even told me about things that were in my dosier. Somebody had even seen me reading the Daily Worker. "Did you subscribe?" he asked.

"No," I replied. "I have seen only two or three issues."

"Where did you buy them?"

"Well, I bought one or two in the United Nations in a meeting and one from a street vendor on the corner of 12th and F streets in Washington."

I asked him, "Didn't you ever read the Daily Worker?"

He replied, "Only in dosiers."

Eventually a man came and asked me to recount again how come I went to that meeting in New York City. I explained it again as I had done a dozen times before just as it was in the file.

He reached into his briefcase and pulled out a manuscript of two pages and said, "Would you please read this and sign it?" It was exactly what I had told him when he first came.

This nearly closed the case. But they went after Bradfield later on the same case simply because he had correspondence with the Committee, which he couldn't find. So I sent him a detailed abstract of my file and this freed him of further trouble. Then the matter was brought again in 1956 to Assistant Secretary E. L. Peterson, who dismissed it most summarily.

What a colossal waste of money this whole affair was! I can recall the most interesting statement by Dr. M. A. McCall, one of the grand men of the Department. He told the investigator, "If Kellogg is under suspicion - put me in prison right away."

In 1943 the Soil Science Society met in Cincinnati, November 10 to 12. The program of Section Five was not especially good. I spoke briefly on our wartime work in the Soil Survey. We didn't have a staff meeting since I planned to have one later in Lincoln.

Thorp had told me that Throckmorton was not especially cooperative with the Soil Survey, partly because I hadn't been out to see him.

So on December 12 I took the train to Manhattan, Kansas. The last part of it was a miserable ride and I sat on my suitcase between the cars. Trains were terribly crowded in those days. It was very cold outdoors and very hot in the hotel. I had conferences with Throckmorton and Dean Call and everything seemed to go along all right. I came around to Milton Eisenhower's

house to call on him according to arrangements made beforehand through correspondence. But he wasn't at home.

One evening I talked to the agricultural students. I returned by train on December 15 to Memphis, which I reached at 10:30 a.m. 3½ hours late. That evening I took the train to Knoxville and it was nearly 2 hours late.

Sherman Johnson was in Knoxville and we had conferences with McAmis, H. A. Morgan, and others about the farm analysis and especially the fertilizer study of the post-war planning committee. I got back to Washington December 21. I talked to Salter then about cooperation with TVA and other matters, especially fertilizers, but nothing came of it.

Sometime in November or December an officer had come to me from the Navy Department and explained the problem they were having in training naval fliers to recognize land patterns that would tell them where they were if they lost their way. We talked about such a course and what it would contain. At the end, he insisted that I give it.

~~During December I worked on this lecture and one for Vassar.~~

draft III, page 203

Insert lost TP page 243 about Geographical society.

1944. During 1943 and especially 1944 we made arrangements with several of the state experiment stations for joint employees. Some of these worked as part-time assistant inspectors but most as full-time chiefs. By this method we were able to pool our half-time funds for a federal employee and half-time funds for a state employee to have a full-time experienced man. It was better for the man too because he had a continuous headquarters. Under this arrangement several young men were able to complete their graduate work. This was an excellent scheme for the time and strengthened the Soil Survey a great deal. We also placed some of our permanent men on cooperative, federally controlled positions with some of the state experiment stations in the Tennessee Valley as a way of getting TVA financial assistance.

By 1950, however, these arrangements became awkward because the unlike systems of automatic promotions in the Department and the colleges were difficult to administer.

I wrote a piece in 1943 that turned out well, <u>What is the War doing to our soils?</u>, which was published as the lead

article in "The Country Gentleman" for March, 1944.

In the middle of January I gave again the lecture on rural patterns to the Naval fliers at the American Museum of Natural History in New York City and then went on to give a lecture on Soil and people at Vassar. I also had some seminars with the students. They were considerably above the average of comparable students in the Land-Grant colleges.

During January, and the remainder of the year for that matter, I spent a good deal of time on post-war planning, and especially on fertilizer policy. We had set up committees of USDA people and state people at each Land-Grant college to develop estimates of production potentials and also of the fertilizers - nitrogen, phosphoric acid, and potash - that farmers would be using during 1950. These were based on the results of the best managers and recent research, with full consideration to the proportion of farmers that could be expected to adopt the best recommendations. When the results came in, adjacent states showed greater variation than could be accounted for in any way

except by differences in judgment. Yet we were able to assemble national figures that were reasonable when tested by any available method.

Although these figures looked entirely reasonable to me, to Sherman Johnson, and to other members of our committee, the fertilizer industry, actually represented by Frank Parker in the Department, took violent exception. They even said that we knew they were wrong but were making them high to justify recommending that the government go into the fertilizer business! (These critics excused Dr. Johnson since he was an economist and was not supposed to know anything about fertilizers. But they were very bitter against me and even tried to get me dismissed from the Department. But apparently they had to wait their turn after Hugh Bennett! In 1951 the actual consumption figures on fertilizers used in 1950 were almost exactly what we estimated they would be, except that potash was a bit low due to short supply.)

On the first of February and running through to May 15 I again gave the course in soils. I didn't want to do it but several insisted so we arranged one 2½-hour session each week at my home. This was the last time I gave a full course in the Department's graduate school.

From the 17th to the 22nd of February we held a senior staff conference of the Soil Survey in Lincoln, Nebraska. I now had in the back of my mind a firm plan for revising the Soil Survey Manual. For the next five years I pushed the staff very hard to develop a series of committee reports on about half of the subjects to be discussed in the Manual. Each one was circulated to leading soil scientists and tested in field operations. At this same time I gave a public lecture, Long-time problems of land use, in which I strongly emphasized the importance of local autonomy in land use.

Beginning in the spring of 1944 and continuing indefinitely, I gave a great many lectures on gardening to community garden clubs and to a large number of specialty garden clubs, that is

about soils and soil management for roses, iris, chrysanthemums, and so on. Over the years this became a bit of a task but it was one of the few things that a soil scientist could contribute to the people in the Washington area. I made up whole series of cards so that I could shuffle them to get about ten different lectures depending on the group. A few lectures were more formal and several were actually published later.

At the end of February I made a fourth and last lecture to the Navy fliers on rural patterns since their instructors thought they could carry on after listening to four of them.

insert p 231a

The volume of correspondence was now reaching very high proportions and one girl could not do it all. Each one of our special war projects required a great deal of individual attention to say nothing of TVA and the regular work.

Meanwhile Frank Parker was busily at work trying to discredit the fertilizer estimates for 1950. He got A. L. Mehring and Byron T. Shaw of his staff to write a completely ridiculous "counter prediction" by projecting the presumed relationship

Near the end of February I had a letter from Macmillan that the Armed Forces Institute wanted to bring an edition of The soils that support us. This took a lot of correspondence off and on through the year since the book had to be reduced somehat in size and questions and answers had to be prepared for use with students. Years later, many young men and a few young women told me about getting this text as "Educational Manual EM 325". It was used in the Army, Navy, Marine Corps, and Coast Guard. As I recall now about 100,000 copies came out in this paperback edition. The royalties later amounted to about $1,200 based on five or six percent of the cost of manufacturing the book.

between farm income and fertilizer sales. Both Dr. Johnson and I pointed out that there wasn't the slightest reason to believe that the past relationships would hold.

At that time Mr. W. A. Minor was an assistant to the Secretary and spoke harshly about Parker working against the Department. But later somebody got to the Secretary or to Minor and Parker pretty well made his own way for the industry. He had been able to get the War Food Administration to appoint a committee of college people to advise on fertilizers for use in the war-food effort. So far as I know they did a good job in harmonizing contrasting state recommendations and in advising on allocations. I don't recall all of those on this committee but Ralph Cummings, Richard Bradfield, and Yoder were members. Frank was in some conference with these men and told them how perfectly silly were the figures that Dr. Johnson and I had developed. He also was able to get established a National Post-War Planning Association. How he was able to charm these first-class men into false statements I'll never know.

In early April I spent a week in Knoxville mainly on TVA work, including conferences with some of the experiment station directors and with the Board. TVA was working up estimates and plans for a large plant at Mobile, Alabama, to produce concentrated phosphatic fertilizer during the War. Afterwards it could be turned over to a cooperative. The American Farm Bureau was pushing this plant since the phosphorus was needed badly and because concentrated materials were much cheaper to farmers. Even at the same factory cost the new fertilizer would cost considerably under half for transport.

At the end of April Macy Lapham retired. I knew that he had no hobbies. The Soil Survey was his whole life. Our cartographers drew up a very fancy diploma for him and we were able to get the Secretary to sign a special diploma-like appointment as a collaborator. We furnished him a part-time secretary and paid his expenses on a few field trips. For years he came to the office regularly and helped our men out a good deal. He wrote a little book of his experiences called

Crisscross trails: Narrative of a soil surveyor, which was published in 1949.

Dr. Drosdoff, who had worked in the Soil Survey, was already doing splendid work in soil fertility on the tung trees of the South. In April, 1944 I tried very hard to get Parker and Salter to set up Dr. Drosdoff with a special laboratory for basic work in soil-plant relations comparable to the status of Dr. Sterling Hendricks. He would be available to help the other researchers in soil management diagnose their most difficult problems. They agreed to do this and I wrote Matt about it. But then Matt sent me a copy of the ridiculous offer Parker made to him. I advised him not to even consider it. Despite all of the talk and the many meetings with collaborators, Parker and his men did no fertility work of any significance. One day in a fit of pique, Salter said to me, "Why is it that with a whole division of soil-management people, whenever there is a public lecture to be given on this subject you or I have to give it?" He said he wanted a frank answer so I gave it to him.

I think Salter believed me but he was too weak to act. This was his greatext failing: He didn't want to hurt anybody's feelings, except possibly those of close friends whom he could count on. Increasingly I was doing a lot of work on the speeches Salter was giving as well as on my own.

It was a great pity for the soils work of the Department that this arrangement for Dr. Drosdoff was not made.

In May we had a full-dress post-war planning conference. (In addition to my consulting work with TVA and other agencies, I was then working on about 18 committees, including the Central Post-War Planning Committee and two of its subcommittees, one of which I chaired.) One can speculate on how much good the post-war planning work did directly, but indirectly it was one of the most important intellectual activities ever sponsored by the Department. Many men had an unusual opportunity to see all facets of American agriculture in relation to one another. Had the ~~xxxxx~~ same intellectual atmosphere been perpetuated a

higher proportion of them would have staid in the Department. Still most of them continued to work in agriculture.

By early May the fertilizer estimates were in quite good shape. Parker had suggested that we ought to meet with the College Committee advising the War Food Administration. A meeting was scheduled in Dr. Johnson's office at 8:00 a.m. about May 10. The previous evening, Parker said that he thought it would be a good idea if he and I let them meet alone with Dr. Johnson. I told him, "That is all right with me because I have a lot of other things to do."

The next morning I went to Dr. Johnson's office a bit after 9:00 a.m. for another meeting on another subject. He asked me, "What happened to you for the 8:00 meeting? I held if up for awhile but we had to go ahead." I said, "Last evening Parker and I agreed not to come so that the college people would talk to you perhaps more frankly than they otherwise might." Dr. Johnson ~~looked~~ looked puzzled and then said, "Why Parker was here all the time and took part in the discussion in his unique way."

That evening Parker came by as I was working in the garden and gave me a real cock-and-bull story. Of course this was a lie too but I made no comment. News of this event got around and didn't help Parker get the confidence of people outside of industry.

In early July I gave some lectures at the University of Illinois to a land appraisers school. These were reproduced under the title, Under all is the land. A curious incident happened here. While this was going on Professor Burlison, Head of the Agronomy Department at the University of Illinois, was rushed to the hospital with a heart attack. Dr. R. S. Smith and others on the staff assumed that he would die. Dr. R. S. did his best to make me promise that I would come there in his place if the position were offered to me. And he was quite sure he could wangle it. Of course I refused to answer. I said, "Let's wait and see what happens." Burlison recovered and held his position far too long.

I used this occasion to go to my old home in Ionia County and close the deal for selling the little farm that had been such a nuisance.

Because of Baldwin's near incapacity I had to put Ableiter in charge of correlation. I was able to persuade Dr. Carleton Barnes to take Ableiter's place for soil survey and interpretation. I tried to find a good assistant for him but I was unable to get a man that I felt fully satisfied with so used a man of lower grade already on the staff.

Near the end of July the joint Department- Land-Grant Soil Survey Committee met in Cincinnati. With some further work on it, the report was published in the Journal of the American Society of Agronomy for December, 1945.

In early August I had a session with Schoenmann and Veatch about the publication of a whole group of cooperative soil surveys made in Michigan under Veatch's direction. This was about the only time I ever saw Schoenmann so put out with Veatch. He finally took a sheet of paper and said, "I'm going

to list the counties and you are going to give me the dates when you will have the reports done." Veatch gave him the dates and we worked up a beautiful plan. As I recall Veatch might have done part of one report but when the cartographers got the field sheets, they found no way to make maps from them.

During the next ten days or so Veatch, Muckenhirn, Simmons, Nygard, and I met at Houghton, Michigan. We took a winding trip to look at the principal soils across the western part of the Upper Peninsula of Michigan, northern Wisconsin, and up a ways into northern Minnesota. Veatch talked just the way he did with students and called out the names of the plants and the rocks. Iver Nygard had never been out with Veatch before. For the first day or so he let Veatch go but after that he told us what the real names were of the plants and the rocks. But Veatch was a good scout. He knew that he had met more than his match and at the end of our trip in Milwaukee, I believe it was, I asked Veatch what he thought of Nygard. He replied, "Iver's a pretty bright boy for a Swede." We had been using Muckenhirn

part time as an assistant inspector. At that time he was very promising and did some good work. Yet I noticed on this trip that he tired very easily. Every evening after dinner he went to bed immediately. Probably this was the reason he left soil science and went into administration for which he had considerably less intellectual talent.

In September Lucille went to Clarksville, Michigan, where her father was teaching. Her mother had had a serious operation for cancer and needed care. She made arrangements for someone to help and returned to Washington. My mother came to our home with her. Although she had planned to stay for a month she was called back because my father had a stroke and needed care.

In September I went to Knoxville for the usual conferences on the soil survey program and the many activities of TVA, especially the test-demonstration program.

Then at the end of September we had another meeting of the Valley States Conference in Birmingham. Here I had a good talk with George Scarseth about the whole fertilizer problem. We agreed

that Parker was lined up completely with the industry and was doing a great deal of harm by confusing the college people, such as Bradfield.

Near the end of October the draft was made of the Fertilizer Committee of the National Planning Association. At least I find a draft copy dated October 31, 1944 with my corrections and notations on it. It's a shame that men like Truog and Salter lent their names to this piece of propaganda. Yet it did get considerable improvement by Dr. John D. Black, T. W. Schultz, and others (partly with my unofficial help) before it was published as pamphlet No. 42, February, 1945.

I had some long correspondence with Prof. Truog and others about the many errors in the draft report. Among them were ridiculous figures, actually quoted as "statistics", given by the companies of their plant capacity. I talked with a few plant managers and found that these figures were generously based on having all the bins

full! No plant could operate with anything like all the bins full. In 1944 fertilizer companies could sell at a good price every ton that they could make. Their total production never came near their claimed capacity. I think these men fudged these figures in order to claim that they could produce a very high amount of fertilizer if it were needed. Most business men of that time fully expected a deep depression immediately after the War. They took no account of the many counter cyclical measures that had been passed by the Congress earlier such as social security, unemployment insurance, old age assistance, the securities and exchange commission, and minimum wage laws.

In November I spent quite a bit of time working on the Messenger Lectures to be given at Cornell. Because of the War we didn't have a Society meeting in November in either 1944 or 1945.

During the autumn, I spent many evenings and Sundays planning a more permanent garden as an outdoor living room. We ordered many shrubs, trees, and other ornamentals.

Just before Christmas 1944 the War Food Administrator and the Secretary agreed on a Committee on National Fertilizer Policy. Certainly Parker hoped to get our estimates killed; but this didn't work either, even though I was not asked to serve on the committee.

I recall that ~~sometime~~ in January 1944, I believe ~~in November or December~~, I gave the annual lecture at the American Geographical Society of New York on Soil and People.

Change to January, 1944

This was about the worst audience I have ever had. It was made up mainly of oldish men and women who wanted to improve their minds. They didn't have enough money for the opera and felt movies were beneath them. I tried every trick that I knew to get some life in that audience. Nothing worked. I felt like shouting "Fire" or any one of two or three four-letter vulgarisms. Instead I finally braced my feet and just read it to them.

1945. As a by-product of my gardening lectures, I discovered that gardeners had no way to weigh things so I conceived of some tables that would translate fertilizer recommendations given in pounds per acre to pints, cups, tablespoons, or teaspoons per hundred square feet, per 10 feet of row, and for individual plants at different spacings. Fortunately an old college roommate - Frank McCauley - came along about then selling scientific equipment. He gave me a nice set of balances to get the necessary data on the apparent specific gravity of the several materials. I developed the tables in manuscript form to hand out. One I gave to Gove Hambidge who gardened. He insisted that the tables and their explanation should be published as a leaflet. By 1960 I suppose the Department had distributed around 1,000,000 copies of it.

A curious incident happened with this circular about 1959 or 1960. In one of the many reprints, the information office of Agricultural Research Service replaced my name with the name of a Division in ARS that had nothing to do with it. I didn't

care about having my name on it but I insisted that the Department remove the substitute. Of course, with government publications there is no financial penalty for plagarism if one is inclined to do it. I have had lots of plagarism but this is the only overt one within the Department itself.

During the winter I saw Dr. Guy D. Smith while on leave and offered him O'Neals job as inspector for Missouri, Iowa, and Illinois. No one trusted or respected O'Neal. Then in April, he asked to transfer to SCS the next summer. What a break!

My correspondence with Dr. Black was now becoming very heavy. I spent quite a bit of time on his famous paper on submarginal land, which was partly an answer to Parker and the fertilizer industry. This ran on into February in order to look over the second and third drafts. Dr. Black suggested I go on as joint author with him but I thought it would be better not to because than the fertilizer people could claim that I had prejudiced him.

All of a sudden the staff got word that Dr. E. C. Auchter had resigned February 3, 1945 to go to Hawaii and that Dr. Vince Cardon

would take his place. When this news hit the Plant Industry Station, the day became an informal holiday. Everyone was saying eagerly, "Have you heard the news?" It was an enormous release of tension from Dr. Salter down to the greenhouse help. Dr. Cardon was a sympathetic, thoughtful, objective man - almost the reverse of Dr. Auchter. As it turned out Dr. Cardon wasn't the best administrator because he went through such agony to make up his mind. And after he had reached a decision he couldn't help keep thinking and talking about the disadvantages of the course he had decided upon.

The Committee on National Fertilizers and Lime Policy released its report on February 3. Despite Parker's effort the estimates made by Johnson and me were included, and the statement was not too bad.

Near the end of February the American Farm Bureau Federation released an excellent statement by George D. Scarseth - A national fertilizer program. This also included our estimates and supported the general ideas of the Tennessee Valley Authority.

In March I spent the week of the 18th to the 24th at Cornell. I gave three lectures - the endowed Messenger lectures - under the general title, Soil science and human welfare. The lectures seemed to go very well and I enjoyed myself immensely. I recall having a nice bedroom in the very top of the tower of the law building.

Afterward I had a good deal of trouble about these lectures. The arrangement was that I would get my honorarium when I turned in a publishable manuscript. I had the manuscript in good shape, turned it in at the end of the last lecture, and received my honorarium. While I had been working on the lectures the previous winter, Dr. Bradfield told me what a wonderfully simple lecture on physical chemistry that Professor Peter Debye of Cornell had made. It was printed and he gave me the reference. I looked it up and found that it was extremely mathematical. I wrote to Bradfield about it. He then looked up the printed version and admitted that it was completely different from the lecture as given.

To complete the story here, after I returned home the editor suggested that I write about as much again as new material in a

tone of appeal to people without much education. He even mentioned two or three popular "screwball" books. I agreed to add some illustrations and to make some simplifications of the first lecture. He even got Marlin Cline to go over the manuscript and got his agreement that it should be greatly expanded for a popular audience. By this time I had too many other and more important things to do. If I were to write such a popular book I should certainly sell it to a commercial publisher with appropriate outlets. Furthermore, it seemed almost dishonest to revise lectures so drastically for publication. After corresponding for several months we reached an impass. So I was disappointed that this little book didn't come out as had been agreed upon. I think it would have gone well. I used the material a bit later for a Sigma Xi lecture.

While I was at Cornell, Dr. Bradfield arranged a seminar on the fertilizer estimates. What a rip roaring seminar that was! Right off the bat a reactionary officer in the Grange-League Federation by the name of Millimum (?) attacked this tentative report as a "communist" document that aimed at getting the

government into the fertilizer business! He didn't know me or know that I was going to lead the seminar. I went after him real hard and he couldn't support a single statement he had made.

I returned home on a Sunday and early Monday morning, March 26, got word that my father had died in Michigan. I took the afternoon train and got to Ionia about noon on Tuesday. Lucille came the next day. He was buried with a full Masonic funeral in our village cemetery at Palo. My mother was extremely talkative as a person might be after release from great fear and strain. She must have been through an enormous ordeal with his continual threats and complaints. I returned at the end of the week and Lucille staid about a week longer.

In March Parker and his staff released a special report on fertilizers and lime "at the request of the Committee." This was a confusing report and not well written. Also Parker had Adams and Mehring "make an analysis of BAE data relating to fertilizer consumption in 1950." Of course these were not obtained by the BAE but by the Post-War Planning Committee with the help of state

committees made up mainly of soil scientists and agronomists. Dr. Johnson tried to work with Parker's group on it but found it impossible. Besides trying to discredit the estimates, nothing was said in this report about the very important interactions of fertilizers with other practices. (Twenty years later Parker still was unable to understand the principle of interactions.)

April 12 our country and the world suffered a catastrophic calamity - President Roosevelt died at his winter cottage in Warm Springs, Georgia. I recall that I was working in the garden. Lucille came out and said, "The President is dead." I remember asking her, "The President of what?" She said, "The President of the United States." He had been such a good president for so long that it didn't seem possible.

Had Roosevelt lived, I believe the post-war period would have been much easier. Later people told me from all over the world that nothing much was done that day. Even way out in the Soviet Union, groups got together and mourned his untimely death in such a critical period.

April 17 Senator Hill introduced the Bill to establish a National Fertilizer Policy and Program. This would provide for the needed Mobile plant and extend the test-demonstration program. The fertilizer industry and Parker opposed it bitterly. War ended befor the Bill was acted upon and it died.

In April I had a great deal of correspondence with Bradfield about the final report of the joint USDA-Land-Grant Committee on soil surveys. I also saw Dr. Black quite a bit on the fertilizer problem and on the TVA work that interested both of us.

In this month Dr. Mark Baldwin finally retired from the Soil Survey.

In May Parker got Jacob and Adams on his staff to prepare another paper to show that in most instances the ordinary low-analysis superphosphate would be cheaper to farmers than the new concentrated superphosphate!

Another point that irritated me a great deal about these "reports" of Parker's staff was his insistence on the use of the

old inaccurate trade term "plant food" instead of "plant nutrient" or "fertilizer". We had stopped this in the Soil Survey years ago. Parker did a lot to perpetuate this old misleading term. (The food of plants is synthesized by the plants themselves from nutrients. Plant foods are organic.)

On May 6 I went to Ottawa, Canada, to take part in the first national soil survey conference of Canada. I got in my hotel just before the great celebration at the news of the Allied victory in Europe. We had a fine meeting but I did not envy Dr. Alf Leahy his job. Each provincial prima donna wanted coordination but each expected all the others to change. They seemed willing to accept many of the terms and definitions used in the United States Soil Survey but by no means all of them.

One of the men from Quebec was unable to speak English. Those from other parts of Canada were disgusted with him. None of the men from outside of Quebec felt any need to learn French although both languages were official.

I had to be very careful in statement since the Quebec soil scientists were quite eager to follow our lead regardless of the other provinces. They copied down everything I said.

At the conference, I urged the importance of ~~cooperation with~~ soil survey interpretations to go along with the classification and the importance of cooperation with those in the laboratory and those doing field experiments. Afterward I sent Dr. Leahy all the committee reports we had developed since the Manual, glossaries and other publications.

Before I came, Leahy had asked me to speak at a banquet of the Canadian Agricultural Society. I explained to him the red tape involved in getting a foreign speech approved during war time and suggested instead that he schedule an "informal" panel. He got the point. Three others spoke about two minutes each and I took a bit over 40 minutes.

We had a large crowd. Somehow I was feeling well, and knew enough of the people to personalize my talk quite a bit. My theme was the influence of the environment on the development of Canada

as settlement went west, along with half serious, half humorous asides on the influence of the United States in modifying the British and French inheritance. All my notes were on a slip of paper smaller than an ordinary playing card. I've often longed for a tape recording, because I doubt that I could ever do so well again.

The House subcommittee on appropriations for agriculture was very rough on the Soil Survey. Partly, this resulted from inept presentations by Salter and Parker. (Salter always professed great regard for me; but still he handled the hearings as Auchter had done.) Even more important, Bennett and his boys, got old Judge Tarver, chairman of the subcommittee, to work against the Soil Survey. The funds were cut only a bit but we were told to limit our work to the special war-food problems. But the old boy from northern Georgia paid the full price in the primary a year later.

I got around this problem by placing more of our men working in the Tennessee Valley on the state experiment station funds, which were reimbursed by TVA. Then also I made a few other states

collaborators and got some more funds for special research from other agencies.

Shortly after returning home from a visit to Ottawa, I received a curious letter from the local Soviet Embassy, as follows.

May 18, 1945

Professor Ch. E. Kellogg
Experimental Station
U. S. of Agriculture Department
Beltsville, Md.

Dear Professor Kellogg:

From June 15 until June 28 in Moscow and Leningrad festivities will be held in celebration of the 220th anniversary of the Academy of Sciences of the U.S.S.R.

The Academy of Sciences of the U.S.S.R. asked me to inform you of the above and is inviting you to participate in the anniversary festivities.

The Academy of Sciences would appreciate it if you inform it at an early date of your acceptance.

Sincerely yours

(Signed)

N. Novicov
Chargé d'Affaires

P.S. Travel expenses and the expenses of the sojourn in the U.S.S.R. will be paid by the Academy of Sciences of the U.S.S.R.

Naturally, I wanted to go. Dr. Vince Cardon got permission for me to do so. Yet it was a job to make arrangements so soon. We were to leave New York, June 7. The Department of State was a bit sticky

But Miss Jonas - travel officer in the Foreign Agricultural Service - knew all the warm places and I just made getting all the visas the day before I was to leave. But State would not give me a letter of introduction for the visas from the countries we might be travelling through. I had to get a personal passport and the introduction letter from the Soviet Embassy!!

At the Embassy I was told to get in touch with the Soviet Consulate in New York. The plane that took their delegates from Moscow to the first United Nations Conference being held in San Francisco would take us to Moscow.

I took the evening train to New York and got there in the early morning of June 6. I had great difficulty in reaching the Soviet Consulate. New York was terribly crowded but my friends at Macmillan Company got me a room. I also learned that others going were at the Harvard Club. According to instructions I gave up my hotel room about 4:30 p.m. and went out to the airport. The others were there. But it seemed that when the Americans wheeled the Soviet plane into the hangar for servicing, they cut the corner too short

and broke off a wing'. Obviously we couldn't go. By this time I knew I couldn't get a hotel room in New York City. Dr. Shapley told me that he could get me one at the Harvard Club for the next night but not for that night. In a few minutes a man in a brown suit came up to me and said,"Do I understand that you have no place to stay tonight?" I assured him that was the situation. He said, "My wife and I are just down here seeing some friends off and we should be glad to have you as a guest." Naturally, I accepted. They drove back to town and took me to a very fancy Russian restaurant and then to their mid-town apartment. For some reason I had an attack of hives in the middle of the night and, to my embarrassment, had to awaken them for a little vinegar.

After a leisurely breakfast I went over to the Harvard Club. There were 15 Americans and one Mexican ready to go. In addition Edward Condon was there and hoped to be able to get a passport. (He didn't.) Dr. Bronk and Dr. Shapley had had a lot to do with the White House and after much calling President Truman agreed to have an American plane take us to Teheran with an A-1 priority.

They also fixed up a fine parchment greeting from the American Academy of Sciences. We all realized that it would be difficult for 16 professors to reach agreement on details so we elected Shapley as our chairman and spokesman - a most fortunate choice.

We were up at a little after 3:00 a.m. June 10 and checked in at the plane. A thoughtful sergeant gave us Arctic flying suits, since there was a delightful rumor that we might return over Siberia. I was off on my first airplane ride at 6:25 in the morning.[1]

footnote [1] See Notes on visit to Soviet Union. 1945.

These notes give only descriptions of the soils and the scientific sessions. We had supper in Santa Maria, breakfast in Casablanca, and June 12 in Cairo. Some of us visited the pyramids and the main mosque. I also met the Agricultural Attaché and the Egyptian Undersecretary of State for Agriculture. In the evening the scientists of Cairo gave us a banquet. The room was so beautiful that several drank the water and ate the melons. Later these got dysentery. We went on to Teheran and stayed all

night. On June 14 the Red Army took us to Moscow with a brief stop at Baku. Some were very ill by then. Fortunately one of our number was a physician .

There were many highlights to this trip that reminded me of Aladdin and his wonderful lamp. We were treated most courteously indeed.

We had a meeting in Moscow on June 18 of those present who were members of the International Society of Soil Science and issued a call for its revival. (See Soviet notes.)

At an early cocktail party at the American Embassy Averill Harriman, our ambassador asked me to come and see him just before we left. We had a big banquet to wind up the Academy sessions on the evening of June 30. The next morning I learned that we were to start for Alaska early on the second of July. So I called Mr. Harriman and he told me to come right over. He had explained that he couldn't talk with each of us and so had chosen only one. He said, "I see from your record that you are in work requiring careful observation." He asked me about 12 or 14 questions about other

other members of the group and about what I observed. (I did get on one collective and one state farm.) I felt sure that he was interested in only three or four of the questions but I didn't know which ones.

He had dismissed all his people and after the questions gave me a long lecture on the Soviet Union. He explained why we would have trouble with them and what the trouble would be. All of this was confidential and not put in the notes.

July 2 we flew to Novosibirsk and then on through Kirensk, Yakutsk and Seimchan to Fairbanks.

We had an excellent pilot and crew. We took the second of July, the third of July, and two fourths of July to get to Fairbanks. I had a nice field trip around Yakutsk with a Russian driver who spoke neither English nor the local language. Although dog tired I visited the University of Alaska and got to look at some of the soils near Fairbanks. After lunch we headed toward Minneapolis, a military airline terminal, and came straight through

When we arrived there about 1:30 p.m., we were through with the military and of course none of us had a priority for commercial flights. One of the men called the White House for a priority. Immediately one of the others and myself took a cab to the railway station. We both got berths to Chicago on the Blackhawk. I was so tired that when I bought my ticket I could not add the prices of the railway and Pullman tickets.

My companion was a physician and have me four sleeping pills, two for each night. I don't think I have ever enjoyed a bed so much. Then in Chicago, I just went around in the line at the Pennsylvania window. The third time around I got a roomette. So when I got back to Washington I was feeling normal again. Thus I had a trip around the world in fine style for a personal cost of about $200.

When I returned I found that the report of the Joint Committee on Soil Survey, which emphasized a state soil scientist in each state for SCS, had been approved by Secretary Wickard.

On June 30 C. P. Anderson had been made Secretary of Agriculture and J. B. Hutson Undersecretary. Charles F. Brannan had been appointed Assistant Secretary earlier.

The draft post-war planning report containing the fertilizer estimates intact had been released.

Frank Youngs had retired with the hope of recuperating from ulcers on a ranch. I made Ray C. Roberts acting and I kept him this way for quite a while, in the hopes of finding a stronger man, before he got the full title.

Thorp had gone out to the Pacific for some direct aid on military intelligience.

Marlin Cline had been borrowed from Cornell University to initiate a new phase of the military intelligience work. We called this The World Soil Map Project because the soil maps themselves were not classified. At first I had several conferences, along with Marlin, with the Geological Survey and the military people. After that he carried on by himself very well.

On the way back from Moscow I resolved to make every effort toward a strong International Society of Soil Science, but more than that to develop a central group that would know us in the United States and one another. This occupied much of my time for the next 15 years.

Almost immediately on my return I was simply bombarded with requests for lectures and articles about the Soviet Union. I accepted as many as I could, which was only a small percentage. Over the next 18 months I spoke to the Department as a whole and to bureau seminars. I spoke at the Cosmos Club, to service club groups, and to women's clubs. At every one of these the crowd was large and eager. Many serious people had questions and they hoped that the answers would be favorable. In 1945 and 1946 the prestige of the Soviet Union was much higher in the United States than they appreciated.

I was also asked to the State Department, Office of Strategic Services, and elsewhere for "debriefing". At SOS one man asked: "Did you fly along the Siberian railway?" I replied that I had.

"Were there many trains," he asked. "Yes", I said. Then he asked, "More than usual?". I explained that I had been there only the once so I couldn't say. Then he asked,"How many trains went per day?" I could not answer but explained that one of our group had asked the same question of our host at Novosibirsk, that I had interupted by saying, "Don't anwer that". "Why did you say that?" my companion asked. I replied, "Because you don't need to know and the Japanese should like to know." This SOS boy was cool after that.

A. M. O'Neal transferred to SCS in August. What a relief to get him out of the Soil Survey.

I think it was in August that Henry Wallace asked me down to his office for lunch. At first he sparred around a bit to see whether I was a Russian hater, which he conceived Secretary Anderson to be. He too had many questions. Then he gave me a lecture on the Soviet Union. He predicted the troubles we were likely to have and why we would have them. Actually his lecture was identical to Harriman's. I learned a great deal from this experience because soon afterward the press pictured Harriman as a get-tough-with-the-Soviet Union man and Wallace as a Soviet lover.

Despite all these requests I decided I would write the way I wanted to and planned seven feature stories under the general title, A scientist visits the Soviet Union. When I got three of them done I took them down to Eugene Meyer, editor of the Washington Post. On the basis of the three he accepted the lot. And I finished them as soon as I could with a million other things to do. When I took them in he said he would send them to the Reader's Digest. I predicted they would turn them down because everything they were including about the Soviet Union was extremely anti-Russian. (They did turn them down.)

Mr. Meyer gave me a nice fee and they ran for a week in the Washington Post, beginning September 9.

The War was over in Japan August 14, earlier than most of us had expected.

John D. Black, with some of his former students, began a large book on Farm Management. He sent me the chapters from August until the end of the year and I wrote from one page to about ten for each chapter. (Thus I made up to him for his help in 1943.)

After the Post articles came out I spent all of my spare moments with speeches and writing about the Soviet Union and letters of refusal.

About the middle of September I had some serious arguments with Charles Brannan about TVA. I don't know why he had such a closed mind on it. And he shuddered at the thought of an MVA which I thought would be a pretty good idea. I had been asked to give testimony before the Senate. See Senate Hearings on S.555, 1945, pp 168-182 (MVA) Of course this had to be approved and I stuck only to the facts with no opinion on the proposal. Yet somehow the senators had a list of the right questions to ask so that the force of my testimony was in favor. Unhappily the Bill failed and the area had a hodge-podge of uncoordinated, expensive programs for many years. A wonderful chance was missed in an area where the TVA principle could have been even more significant. But of course TVA was on trial then, not because it had done badly, quite the reverse, because it had done very well.

Near the end of September Salter, Sebril, and I drove to Ithaca for the meeting of the collaborators on the Plant, Soil,

and Nutrition Laboratory. Not much progress had been made except for getting ideas a bit crystallized. The laboratory had gotten a new "soil scientist" - a man by the name of Hunter. He certainly was a poor one and happily they got rid of him a year or two later.

As soon as I returned from Ithaca I went out to Lincoln for a review of our work and discussions on present and post-war planning with Elmer Starch, Sherman Johnson, and others.

Because of antagonism to post-war planning we were now pushing forward to a final report for the Secretary.

On October 6 I gave a long broadcast over NBC as a part of an hour show. This had to be approved all the way around, including the State Department as I recall. The part of it that I recall most vividly was the admonition just before the program began: "The engineer has the script and if you depart from it he will cut the switch." Not every democracy trusts its scientists. I had a lot of favorable fan mail from this broadcast.

Earlier in the year I had agreed to serve as Secretary of the Agricultural Commission at the Founding Session of FAO in Quebec.

Many of us went up together on a special train October 14. I was given a room way up in the tower of the Chateau Frontenac. I had nine professional assistants from the United States and Canada. Dr. Archibald of Canada was Chairman but he had other fish to fry and left nearly all of the work to me. What a rat race! The British delegation opposed me most of the time. The Soviets were much more cooperative. I split the Commission up into seven subcommittees with a secretary for each. (The British spoke of "Kellogg and his seven dwarfs!") I don't think I ever worked harder in my life. Every country had one to many members on this Commission. Nearly every delegate had some little pet idea to press me with. Happily, no matter what I did, Dr. Archibald supported it. (He reminded me enormously of Dr. Henry G. Knight.) The other very helpful man was the Major in charge of the troops assigned by the Canadian government to fix up the committee rooms, move furniture, and so on. He was a great tall man, dressed up in the kilts of his Canadian Scottish Regiment, by the very un-Scottish name of Vladimir Ignatieff.

We finished our last plenary section of the Agricultural Commission at 3:00 one afternoon and I had to have finished copies in the hands of all delegates by 9:00 the next morning. I went directly to my room in the tower and used my assistants as messengers. I wrote the final draft of the report, page by page, and sent it down to be typed and the typed copies were brought back for corrections. A little after 3:00 a.m. I sent down the last sheet. I was finished. During all this, I tried to get people to vote to have FAO headquarters in Northwestern Europe, so that it would not go to Rome where the Ministry of Agriculture was so pro-Fascist.

The next day Archibald's conscience pricked him so that he took me for a ride in the country and we visited a very fine French Canadian family that he knew. (I suppose I have nearly a bushel of files on this Conference but all one needs to do is to get the report as published. Of course that wont explain some of the silly footnotes that I had to put it, such as one for the delegate from Luxemburg that notes that grapevines are included under fruits.

The Soviets had hoped they could sign but at the last minute they got instructions not to because the agreed-upon plan required each member country to supply agricultural statistics.

During the sessions I was continually mindful that , at least temporarily, I was serving as an international civil servant and studiously avoided delegation parties. But after my work was done the Americans persuaded me to come to their last one. Old Senator Thomas was one of the delegates. I recall that Secretary Anderson and several others were pretty well plastered by the time I got there and were singing very bawdy songs along with the secretaries of the delegation. After awhile old Senator Thomas came in with a gorgeous young doll dressed out in furs. In a few minutes she and Anderson were very chummy and they went off together in one of the bedrooms and closed the door. The Senator passed a most uncomfortable 15 or 20 minutes to look at his face before they returned. By then the party was so loud and rough that I really expected the police so I left. Somehow I never trusted Secretary Anderson after this, which was probably very old fashioned of me.

I got home from this task about November 2.

Two or three weeks after that I was up at the FAO temporary headquarters in Washington and who was there but Major Ignatieff

in civilian clothes.

"What in the world are you doing here?" I asked him.

"I'm here about a job," he replied, "After all I'm a soil scientist."

"Well why in the world didn't you say so during all those days we had at Quebec?" I asked him.

He replied, "I thought you had on your mind all you needed and some more."

I don't believe there is another soil scientist needing a job in the world who would work with me for nearly a month and not say one word about it.

In the autumn the National Fertilizer Association got out a special issue in praise of the fertilizer industry and begging everyone to fight against the Senate Bill introduced last spring and its companion in the House.

Earlier, in the autumn of 1943 the preparatory committee ~~Just a little later that winter sometime~~ FAO gave me some money to bring together a small committee to work up a plan for a ~~soils~~

of soil science and management in the UN agency (FAO) to be set up, as a department ~~in FAO~~. I got together Truog, Bradfield, ~~and~~ Pierre, and ~~perhaps~~ Salter. ~~I don't recall, and~~ We fixed up a good statement that was put in the refrigerator by the new Director General of FAO, John Boyd Orr. He was a bit flighty. He and Sir William Ogg, then Director at Rothamsted, had carried on a bitter feud for nearly a generation. Since Ogg was a soil scientist, ~~he~~ Orr hated the subject. So he put soil science ~~in~~ as only a section within a department which handicapped the work for at least 20 years. What a pity!

In December Professor Whitson died.

I gave several speeches on the Soviet Union and did other writing.

W. ~~V.~~ Carter died December 12, a real loss to us.

The general report of the post-war planning committee - Peacetime adjustments in farming was published as Msc. publication No. 595. Our fertilizer estimates had gone all the way despite the opposition.

In December the planning function formerly handled by the

BAE was cancelled and in place of it Anderson set up a Policy and Program Committee. This heralded the end of the great intellectual stimulation of the Department that was initiated by Henry Wallace and M. L. Wilson. Between Christmas and New Years I attended the AAAS meetings in Philadelphia and let Bronk and others talk me into being a national Sigma Xi lecturer for the year beginning January 1, 1947. I found out afterward that when the invitation came it was missent to a man in the Department - a rabbit specialist with a name like mine. He replied that he would give the lectures. "What do you want me to talk about?" The Sigma Xi officers told me about it and said they knew I didn't write the letter and sent another one with a more nearly adequate address.

Throughout the year and for the next three years I carried on a long correspondence with Professor G. W. Robinson of Wales. I tried to get him to let me arrange again for a visit within the United States. But he would not agree. He said, "I could not hold up my head in the United States so long as we continue to get handouts from your government." He was very proud and felt really that nothing had been right since the Old Queen died.

1946. When Dr. Cline was loaned to the Soil Survey this gave Dr. Bradfield an opportunity for a visiting professor so arrangements were made for Herbert Greene to be at Cornell during the winter and with us during late spring and early summer. We were both a bit disappointed that Greene did not make more of these excellent opportunities. In addition to many speeches of my own that I had to do I found myself saddled with some for both Cardon and Salter.

Dr. Bradfield was apparently doing all that he could to encourage station directors to help us with our appropriation but the trouble was the poor presentations made by the Department.

I discovered about this time that Frank Parker, Ralph Cummings, and some others were trying to reduce the Soil Science Society to a division of the American Society of Agronomy and to get the Fertilizer Committee out of the Soil Science Society. I talked to quite a few people and wrote letters about this over many months. I finally told Cummings and enough others for it to get around that if the Soil Science Society were to lose the slightest bit of its present autonomy, I should cash my war bonds and buy stationery and

stamps and put my best effort into organizing a new society for soil scientists. I would not say one word against the American Society of Agronomy but I would make the best arguments I could for a stong, general, soil science society that should try to cooperate with societies for field crops, horticulture, forestry, economics, and engineering. I might not get a very large membership but it wouldn't be for want of trying. I told them that I thought ~~that~~ it would ruin the American Society of Agronomy although that would not be my aim or purpose. This put the end to that kind of talk. Now I think I should have gone ahead anyway.

During January and February I had quite a lot of correspondence with Henry wallace who was then getting a new farm established. Wallace was beginning a farm enterprise near South Salem in New York and had many questions about soil management practices.

Mr. William M. Johnson had come back from the Pacific War after many narrow escapes and joined our staff in February. This was one of the best appointments I had ever made.

During February I fussed with the TVA work, gave lots of speeches on the Soviet Union and began to put down in an orderly fashion my views on world food potentials. I had little realization then what this effort was destined to grow into. I knew only that someone must explain that the world had the potential abundance and that without a great deal of agricultural development in the tropics no peace could be hoped for.

From about February 20 to 24 I held a senior staff conference in Columbus, Ohio just prior to the Society meetings. We reviewed the work of all the committees and got some more underway. We were making steady progress toward a new Soil Survey Manual. Certainly my guardian angel was very kind to hurry me along on this.

Only one really amusing incident sticks in my mind - one that illustrates the extreme provincialism that still handicaps sound work in general soil science. We were discussing the tentative standards for stoniness in soils and the definition of phases to indicate varying degress of stoniness. Bill Latimer made some sort of suggestion based on his experience that had all

been in the eastern states. I thought about his suggestion for a minute or two and one half out loud and one half to myself I mused, "Let's see, how would that work on a Chernozem soil?" Latimer was a bit irritated and said, "I don't know about that. I was thinking of ordinary soils." The conference simply roared and Bill never knew why.

I discovered that after the men had set through this kind of meeting they weren't really in much shape to enjoy the Society meetings. It was a good way to get the men to the Society meetings but inevitably one meeting or the other suffered.

Near the end of March I talked in St. Louis at a special meeting in AHAS on the World Food Potential, but only from notes. insert 2 pg.

I think it was about April that Salter and I had a talk about the very low yields on the fields at the Research Center. Parker, Parks, and some of his other "soil management experts" had made recommendations to the Animal Husbandry people but still the yields of silage corn and the like were pitifully low. I asked him, "Would you really like to get those yields up? If so, you

The Secretary's office had been pressuring Bennett to drop E. A. Norton. In the eyes of the college people and others he was the main trouble maker. Actually, Norton did what Bennett askedd him to. But Bennett did agree to replace Norton with Roy D. Hockensmith as Chief of soil conservation ~~services~~ surveys, ~~now~~ then way down the line under "planning" within "operations" in both the Washington and regional offices. Roy had much better manners than Norton and very much wanted peace and harmony. But he did not understand ~~many~~ clearly of the scientific issues, such as the compelling need for a closed, descriptive legend for each survey, and would not stand up to Bennett on the few he did understand. Most of the regional survey leaders were very poorly trained. So neither the surveys nor the quality of direct supervision over the field workers improved, and the quality of the field soil surveys was very poor indeed, except for the few cooperative with the Soil Survey.

just put Dr. L. T. Alexander on the job and I'll guarantee that he can do it. Lyle knows how to daignose the problem and these boys don't and one must start with diagnosis."

So he called in Dr. Alexander and turned him loose on it. The base saturation was very low. Lyle worked out specific lime, phosphate, potash, and nitrogen applications based upon both morphological and laboratory examinations of the soils. The livestock people didn't know whether they wanted to spend that much money or not. Lyle pointed out that the response would not follow the classical parabolic curve but rather the S-curve that had been worked out in the Tennessee Valley for many soils. He urged them to use the full recommendations on whatever they did fertilize because one half would give them nothing. So they tried it on half of their acreage and the results were astounding. So of course the next year they did the rest and had silage coming out of their ears.

In May I went again to Vassar and gave two lectures and some seminars. I was again impressed with the high quality of these students, one of whom we hired for the laboratory.

Sometime in late spring, as I recall, Vince Cardon had a mild heart attack. He lost his head momentarily and recommended his Assistant Administrator, W. V. Lambert, to take his place. A little later Byron T. Shaw took Lambert's place. These changes were unfortunate. Had Vince had more time to think he wouldn't have made such recommendations. He became a special assistant to the Chief of the Bureau of Plant Industry, Dr. Salter, and went back to be Research Administrator.

In June the Fertilizer Bill came up again especially in the House. This was a very good Bill which I had had some chance to influence. It provided for construction of the Mobile plant, which would ultimately be operated by a cooperative, for pilot-research farms, and for test-demonstration farms to be operated by the colleges. Of course industry and Parker's group were opposed although officially the Department favored it. But the War was over. Many people expected a depression, and the Bill lost.

This month I worked a bit with the American UNESCO Committee.

From the very start, and for many years afterward, the top people in UNESCO had both feet firmly planted in mid-air. Instead of concentrating on the overwhelming problem of general education they tried to be dramatic and thus duplicated the work of FAO and the other specialized organizations of the United Nations. In 1964 this policy still prevailed. My best efforts to get them to avoid it were not good enough.

In June I had my first conference with a man who became very important in my life - M. F. Jurion, Director- General, L'Institut National pour l'Etude Agronomique du Congo Belge. He and his assistant had had a long trip arranged through the State Department within the United States. I talked with them at length about their interests and purposes. The itinerary was a poor one for them and it was too late to get as many changes as I should have liked.

In June Dr. Iver Nygard had preceeded me to Alaska as a member of a team of seven of us that were to make a study of the territory (now state) in view of developing a set of recommendations for an

agricultural research program. I had the further view of getting enough material to produce an exploratory soil map and report.

I believe that it was in the summer of 1944 that President Roosevelt had made a brief inspection trip to Alaska and became convinced that the territory had been neglected. Later, he directed his cabinet secretaries to make budget estimates for research and development plans. The Department of Agriculture had rather substantial estimates for research. When the bureau chiefs defended these estimates before the appropriations committee in the spring of 1946, it became obvious that none of them had been in Alaska or knew anything about it. So the committee provided for enough money to pay the cost for a team to make a study and report both to the Department and to the subcommittee on appropriations for agriculture. Interestingly they sent along Arthur Orr, clerk of the subcommittee as a kind of commissar. We left on the 5th of July.

For the details of this visit see Alaska Journal, July-August 1946 including Some reflections in the quiet of St. Mary's Hospital.

Although strenuous, the visit to Alaska was very fruitful. I got a fair look at the Aleutian Islands, Point Barrow, and Mount McKinley. Dr. Nygard got to the Copper River country and to other outlying places I did not see.

While in Alaska I received a cable from the Soviet Union asking me to write a special introduction to my book, The soils that support us, for a Soviet edition for the chairmen of collective farms. I recall writing this on long periods between islands on the trip to Shemya. (Because I refused a return trip to Moscow, or for some other reason, I don't believe this was ever published.

Another "outside" highlight was the news that Judge Tarver had lost out in the primary, partly for his dirty work on the Soil Survey. Apparently this was interpreted as a dig at TVA.

I became convinced that Alaska had very little promise for farming. Many emotional people had incorrectly pictured Alaska as a great area for small farms. This was bunk. Actually most of the area is too cold, is too steep, or has too shallow a mantle of fines over cobbles for a potential arable soil to develop.

Nygard and I put an outside limit of 1 million acres of potential farm land in the Territory, for which we had enormous criticism, especially from enthusiasts in the Department of the Interior. Actually we were "playing safe". A generous figure is closer to 100,000 acres. Even by 1952 - 15 years later - not more than about 30,000 acres was used for farming. The reasons are explained in the Exploratory study of the principal soils groups of Alaska. 133 pp. illustrated. (map). Agr. Monograph No. 7, USDA. 1951. After all, prices cannot be higher for farm produce than Seattle plus air freight.

My back became very sore on this trip so I stopped at the Mayo Clinic about August 10, 1946. After ~~two~~ four days of examination they put me back in St. Mary's Hospital for a series of treatments. I sent my film on for development. So in the hospital I wrote my journal and the captions for all my black-and-white photographs. This turned out to be a wonderful place to write.

On August 31 I went to Madison, staid all night and had a visit with Truog. I left Madison early September 1 with the usual terrible service on Northwest airlines. I should have been home about 3:00 p.m. but instead it was about 8:00 p.m.

While I was away Bill Latimer transferred to SCS as a result of my hints that he should retire. He did retire in the SCS, but on full salary for about six years. Years later, I learned from Mr. J. G. Dykes the trouble it caused him at the Bureau of the Budget to have men like O'Neal and Latimer on the payroll. Roy Hockensmith did not ask for them but had to report regularly on their "accomplishments".

I found that while I had been away my people in the cartographic section had gotten into a row and had to bring in an outsider to adjudicate it. Mr. McKericher, who was my administrative assistant, had also doubled as leader of the cartographic section. Ever since I had returned from the Soviet Union he had gone downhill rapidly. Everybody like Mac but now he was losing his memory for detail and his alertness. As a young man he had become

engaged to a girl whose parents did not approve of her marriage. Yet she and Mac went out together frequently for years and years. While I was in Moscow she died. I knew I had to get Mac to retire someway and to get a man to head the cartographic section. Both of these took quite a lot of doing. Henry Allanson helped me out by talking to Mac over the next year or so. And I sent out discreet letters of inquiry for cartographers.

Either just before or after the Alaska visit I put the finishing touches on one of my better papers, What is farm research? Here I tried to emphasize interdisciplinary research, followed through on pilot-research farms and to the test-demonstration farms. For some reason the concept of interactions was difficult to get across. I don't know how many speeches I have given on this subject over the years but well over a hundred. Then too, I am satisfied that only college students read the Yearbooks to amount to anything.

It had been planned by my staff while I was away that I should need to go to Atlanta, Georgia to represent the Secretary of Agriculture at a hearing on freight rates for rock phosphate.

Reservations had already been made at the Atlanta-Biltmore Hotel. The plane was a bit late and I got into Atlanta about 6:50 p.m. In the bus going into the city I sat next to a man in a brown suit. After awhile I said to him,"Couldn't they get a piece of level land closer than this one for an airport?"

He replied, "I guess not. What do you know about Atlanta?"

I told him I didn't know much so he proceeded to tell me all about Atlanta. Then he asked me, "Where are you staying tonight?" I replied, "At the Atlanta-Biltmore."

He inquired, "Did your people ask for late arrival? Otherwise you probably wont have a room."

I said, "They didn't say anything about it but maybe they did."

The man introduced himself as Mr. Rothberg and told me that he lived in that hotel. "Don't worry," he said, "I have an extra bed in my apartment and you can stay with me."

Finally we got to the hotel and the clerk didn't have a room for me but said he might have one after awhile so I went on it to dinner. When I was about half through Mr. Rothberg came in and

said, "They wont have a room for you so I've had your bag sent up to my apartment. I've got to go to the office for awhile and should be back by 10:30. Here is the key so just go up and help yourself." And he walked out.

I said to the waitress, "This beats all. This man gave me a key to his apartment and I haven't known him for more than an hour."

She said, "He is the most wonderful man in Atlanta. He is always doing things for people."

After dinner I found the lawyers and worked on the rate case to get my testimony lined up. It was about 11:00 when I got to Rothberg's room. He was reading in bed and had appointments for me to see the newspaper editors and either the Governor of the Mayor, I don't recall. After a little chit chat we went to sleep.

The next day it became obvious that this hearing would go on for days. I told the lawyers that I simply didn't have the time. So they spoke to the Examiner and the opposing counsel for the railways and it was agreed that I could go on right after lunch.

The hearing went fine according to plan, except that the

young lawyer asked a final question that we had not rehearsed, "Since phosphatic fertilizer so increases production and therefore traffic, the railroads could really afford to carry it free could they not?"

Well, of course, the opposing attorney, who was a very sharp man, objected and asked me, "Doctor, how much do you know about railroads?"

I could reply only, "Absolutely nothing," although I knew well this was hurting the testimony. Yet if I had said anything else it would have been hurt much worse.

Thus I was through about 5:00 p.m. I went down to Mr. Rothberg's room and there he was, with still more plans. I explained that I had been away for two months and was very anxious to get home but I supposed that it would be hard to get a place on a plane that evening.

He said, "I should like to have you stay very much but if you're really anxious to get home I can get a reservation." He picked up the telephone and called someone named Mabel and told

her he had to have a seat for Washington that evening. Then he turned to me and said, "You go up to that room where they put your bag and I'll meet you at the desk in about ten minutes. I've a few more calls to make. And don't pay for the room, you didn't sleep in it."

So I got my bag and went down to the desk and explained matters. In two or three minutes Mr. Rothberg came along, picked up my bag, and went out the door. Here he had a car waiting. I said, "It's not necessary for you to take me to the airport."

"I want to", he said, "Just get in." The driver really started rolling even though we had a great deal of time. Out near the edge of the city, he swung the car into the driveway and stopped in front of a beautiful country club. My new friend explained, "I thought we might have time for a little bite to eat."

We went inside and here was a fine dinner all set up for two. After dinner we went on to the airport. My ticket was all ready. I paid for it and got on the plane. This trip reminded me of my experience a year before in New York City. I've liked Atlanta ever since.

About this time Director-General Jurion returned from his tour within the United States and called on me. He wasn't happy with his visit. I tried to explain that very few of our people had been to the tropics and most simply couldn't visualize the conditions there. He told me about some of his problems, one in particular. "At Yamgambi," he said, "Much of the African oil palm has phosphorus deficiency, according to the evidence of foliar symptoms and chemical analysis. Yet when we laid out some experiments using phosphatic fertilizer, the deficiency was worsened. What causes that?"

I told him, "Frankly, I don't know. I haven't seen the plants, the soil, the fertilizer, or the analyses."

He asked me, "Could you tell if you came there and looked over the plantation and studied our data?"

I replied that I might but I couldn't guarantee anything.

Then he said, "Well, would you come if we could arrange it with your government?"

"That," I said, "Is a hell of a question to ask a general soil

scientist who would love to see central Africa. Of course I'd come."

I thought he was simply being courteous like many other foreign visitors, but it worked out fine in the end.

The military people were very anxious to complete soil maps of Okinawa, Japan, and the other Pacific islands while we still had them under military control. So arrangements were made for Dr. Simonson to go out and also many other people during the next five or six years.

Dr. Nygard and I had collected a good many samples of soil profiles along with samples of the vegetation on them. As these came in I made arrangements for laboratory analyses with Dr. Alexander and with the laboratory at Cornell for the vegetation. This work had to be handled as time became available and it was a long time before the analyses were finished. In the meantime we made a report of the findings, which was published in 1949, and prepared a set of recommendations on research to be in the budget for fiscal year 1948. These recommendations were made to Lambert. I recall the great difficulty I had getting him to understand the need for

for farm management research. Lambert had no appreciation of economics. I got Dr. Sherman Johnson to help me and Lambert reluctantly agreed to a small item.

One of our biggest problems in making these recommendations that bothered both Dr. Olaf Aamodt and me, was the relationship of this new research to the Agricultural Experiment Station of the University of Alaska. Dr. Aamodt and I were both ardent believers in cooperation with the state experiment stations. On the other hand it was obvious that the President and the Experimental Station Director at the University were incompetent to administer research. Essentially all the funds they had already were used to support dairy herds and the like for supplying student dining halls. Thus, much as we disliked to do so, we had to recommend that any new funds be under the administration of the Department until the University administration became reasonably competent.

But we were not required to put this in writing. For this I will feel eternally grateful to Mr. Everett Dirksen. At that time he was in the House of Representatives and chairman of

subcommittee on appropriations for agriculture. He called Dr. Aamodt and myself for a special hearing along with one or two of the other team members and Mr. Orr, the chief clerk of the Committee. His questions were good ones and we had a fine hearing. Then he asked us about the administration question of the Department versus the University. I hesitated. He said, "Would you prefer to answer that off the record?"

Both Dr. Aamodt and I explained our predicament as scientists and the possibility of damaging our cooperative work with the states if our motives were misunderstood. He was very nice about it. We did explain it fully off the record. And then Mr. Orr said he would be willing to explain it on the record, which he did very well. So there was some use after all for a "commissar".

When these hearing became known the President and the Experimental Station Director of Alaska wrote bitter denunciations of Dr. Aamodt and me as federal bureaucrats who wanted to dominate everything. They came to the Land-Grant Association meeting and buttonholed everybody. Yet they got no takers and only hurt themselves.

Happily there was no written record of any kind that we had made any recommendation on the administrative question.

It also took me a long time to prepare the Sigma Xi lecture, Modern soil science.

Earlier in the year, Firman E. Bear had published a ridiculous paper in the Scientific Monthly about soils and fertilizers. This was the old "balance-sheet" theory of Justus von Liebig. He said something to the effect that when the soils of Iowa have been formed as long as those of New Jersey, the farmers in the two states would be using the same kinds and amounts of fertilizer. When I read the piece, I simply shrugged because Bear was unpredictable. He could write very good pieces and he could and had written sheer nonsence. In fact, he would write almost anything on any subject, especially if the editor would print his picture along with it.

The editor of Scientific Monthly got a lot of very critical letters for accepting the paper. But many of these were from other kinds of screwballs. He talked the matter over with Gove Hambidge

and Gove suggested that the editor ask me to do an article on all the screwball ideas about soils - the "balance-sheet", the "erosion menace", "plowless farming", and the rest. So he wrote to me. I explained that I should very much like to do it but that at the moment my time was fully committed to other things, especially the Sigma Xi lecture.

November 15 I made a platter for an international radio lecture on the problems of the small farmer. This was to be translated into languages appropriate in central Europe for broadcasting there.

I attended the Society meetings in Omaha, November 19-22. We had already decided that it wasn't practicable to combine these meetings with the national staff conference.

At this meeting I spent a good deal of my time in individual conferences about headquarters, new cooperative projects, and about men for employment. I had asked Whitlock to come to Omaha from Lincoln and made up my mind to offer him the position as head of our cartographic section. At that time he was heading the SCS cartographic unit at Lincoln. Whitlock turned out to be very industrious and resourceful although a bit careless about regulations once in awhile. He greatly increased the amount of work done and reduced costs.

When I returned from Omaha I found a radiogram from Professor Prassalov, Head of the Dokuchaev Institute, inviting me to attend a celebration commemorating the centenary of the birth of Dokuchaev with all expenses to be paid by the Soviet Government. The celebration was December 6 to 20 and a good chance that this would interfere with the Sigma Xi lectures.

I made a serious mistake in judgment on this matter. Instead of writing a formal letter to the Soviet Embassy here, I went down to see them about it in order to explain why I couldn't go. This was in addition to my letter to Professor Prassolov. The embassy people knew very little about it and would not accept my explanation but insisted that, "we know that your government wont let you go." After that until 1956, the Soviets were very difficult. They attacked me in their scientific journals and in New Time. They didn't attack me so much as a scientist but more as a tool of western imperialism and Wall Street. I went to other countries primarily to get them into the clutches of American imperialism.

This wasn't very pleasant because some people were bound to believe such attacks. I assumed later that this killed the proposed Soviet edition of The soils that support us. All this happened later even though I had published in the Land Policy Review, a government publication, a laudatory article on Russian soil science to mark the 100th anniversary of the birth of Dokuchaev, a copy of which I had sent to the Lenin library.

During November and December I wrote a paper for Dr. Salter to give on World soil and fertilizer resources in relation to food needs. This was for a program that I helped to arrange, Section O of the AAAS in Boston. Dr. Salter got a lot of mileage out of this paper. I didn't attend the meeting because I was already committed to give a paper on The natural sciences and farm planning at a meeting of the American Farm Economic Association in Philadelphia at the same time.

On the last day of the year the President proclaimed the end of all hostilities in connection with World War II.

1947. During December and January I completed plans for a long journey to Europe and Africa. The Department paid my expenses to Paris and in Britain but I was a guest of the French in southern France and Algeria and of the Belgians in Belgium and the Congo.

Science for January 3 carried about the most ridiculous of Hugh Bennett's many emotional speeches. Among other things he wrote: "Under the names of peasant, farmer, rustic, and country fellow, these individuals have been synonymous, for generations, with all that is naive, uneducated, and backward." He tried to make the case that since farmers were so stupid they should not be permitted to manage land without a license from the government, which could be had only with a showing of sufficient training and knowledge. (*A la* Bennett, one supposed.)

In his book, The politics of agriculture: Soil conservation and the struggle for power in rural America (The Free Press, Glencoe, Illinois. 282 pp. 1952.), Charles M. Hardin does an excellent job in describing the Bennett position.

Whitlock came in January 17 and took over the cartographic section. Many of the staff did not appreciate the stricter organization and guidance since McKericher had managed it something like a Sunday School.

Again I had a great deal of correspondence with Dr. Black about farm planning, fertilizer policy, agricultural organization, and the like.

Arrangements were made with Cornell University to borrow Dr. Cline again for full-time work. When he left the World Soil Map Project, about July 1, 1946 we had made an arrangement for him to spend one fourth of his time on soil survey work with us, or perhaps it would be better to say to spend one half or more of his time on the cooperative work under a cooperative appointment.

Zera Foster had apparently lied to me when he transferred to SCS in 1939 or thereabouts. He said he would go ahead and complete the manuscript for the published soil survey yet apparently he had done nothing on it and had no intention of doing anything. By the time we discovered this fact the War prevented us sending anyone

to Hawaii. I couldn't find a man sufficiently experienced with tropical soils whom I could trust so we made arrangements for Dr. Cline to go as soil survey party leader and inspector. He staid there several months as I recall and then took odd times to complete the manuscript at Ithaca.

Beginning January 20 I started the circuit of Sigma Xi lectures at Yale University. I then went to Wellesley, University of Maine, and Rensselaer. I worked my way out through the middle west to California, back through New Orleans to Florida, and then up the east coast. On this swing I made the same lecture 21 times! Sometimes the sessions were formal for Sigma Xi members only. Once it was scheduled as an all-college convocation! Of course one couldn't use the same lecture. And then in subsequent weekends I gave it five more times at universities in the middle east. My subject was Modern soil science, written partly as a condensation of the Messenger lectures with some illustrations on slides. Although it seemed to go well I got thoroughly sick of it and after about ten of them I didn't see how anybody could possibly be interested.

Yet apparently they were and the Sigma Xi officers were happy. I received my expenses and $50 per lecture. I had enough accumulated leave so I got my regular salary right along.

On March 10 I was invited by the Bryn Mawr Club of Washington to speak on a panel about the USSR with James Reston and Roscoe Drummond. President Truman had just come out with his so-called Truman Doctrine - a policy for supporting Greece and Turkey against the Soviets. Reston and Drummond were then bitterly anti-Soviet. To them Communism, the Soviet Union, and Russia were exact synonyms. I tried to steer a middle course and point out some of the other aspects including the gallantry of the Red Army, the strength of Soviet science, the friendliness of the people toward Americans, including their extreme gratefulness for American aid, and the extraordinary cruelties and devastation carried on by the Germans. We got into quite a row. The boys hadn't done their homework and even spoke of Metaxus as the present leader of Greece although he had been dead for some time. The audience seemed to be more partial to a middle ground than to their extremism.

We had our staff conference, including much of the committee work at the Plant Industry Station during the middle of March. This also proved to be a bad place to hold a meeting because the committee couldn't meet evenings and because I was called out several times a day.

In April I had another letter from the editor of Scientific Monthly about the paper on conflicting doctrines about soils that I had promised. I had to write to him that I was just climbing aboard the plane for a long trip to Europe and Africa.

A major purpose of my trip to Europe was to get the International Society of Soil Science revised. At the Third Congress in Oxford in 1935, the decision had been taken to have the next one in Berlin. Professor F. Schucht was elected president and, in accord with usual practice, most of the other officers were German, except for Professor D. J. Hissink of Holland, the Secretary-General. Hissink had tried to keep the Society going during the War but after the extraordinarily cruel occupation of Holland by the Germans, he was completely within their control. And Professor Schucht died during

the War. There could be no thought of allowing the Germans any part whatever in reconstituting the Society.

While I had been in Moscow in 1945 attending the Jubilee sessions of the Academy of Sciences of the USSR, several of us discussed the need for the reconstitution of the International Society, including Prof. Demolon of France, Dr. Ogg and Dr. Crowther of the United Kingdom, and several of our Russian friends. A proposal was prepared pointing out the needs.

Prof. Demolon promulgated the rumor that old Dr. Hissink had cooperated with the Germans and could not be trusted. Personally, I did not particularly like the old man because of his arrogance typical of a few Dutch, but cooperating with the Nazis was a very serious charge.

I had found it difficult to get information and at the Quebec meeting of Fao in the autumn of 1945 I became acquainted with Mr. S. L. Louwes, in the Dutch Ministry of Agriculture who had had a high post during the war. He did not know Dr. Hissink personally, but agreed to make an investigation. Dr. Waksman of the United States

was also spreading Demolon's rumor. On April 20, 1946 I received a letter from Mr. Louwes explaining that his inquiries had established no wrong doing by Dr. Hissink.

I then wrote letters to several prominent soil scientists in the United States and abroad explaining the situation. I agreed that Hissink had made errors in judgment under great strain but that evidence was lacking to support Demolon's charges.

We discussed the whole matter at the meetings of the American Society and officers of the Society wrote to several other soil science societies abroad about making a new start during the French meeting in 1947.

Since the French had invited me to take part in their excursions in Southern France and Algeria, it was proposed that an arrangement be made for a meeting in Paris that year. I could represent the Americans. During the intervening time I discussed the matter with several senior members of the Society and with people in the State Department. The correspondence about Dr. Hissink continued to mount but it did not really change anything. I wrote to

Hissink and suggested that we try to meet in Europe. At the American Society meeting in Omaha November 21, 1946 we had further discussions among ourselves and later correspondence with our European colleagues. The French suggested also a model constitution. Since A. G. McCall was a member of the executive committee at the time the war began he gave me a nice letter to delegate his responsibilities to me.

On April 28 I left for Paris, ~~and then went by train to Montpellier.~~ Mail 306 a We arrived on May 1 after two nights without sleep. Since it was May Day, the Communists had a parade. We had a paper-reading session there and then went in buses for field trips that included Nimes and Arles. The field trip ended in Marseille. The next day we took a boat to Algiers. See the Mediterranean Journal for 1947 for all the activities in France and Algeria.

For the first time I met Dr. C. H. Edelman at Montpellier. I was very much taken with him and we cooperated for years later. Certainly he was a great soil scientist and teacher of soil science. We arranged for a get together in order to discuss plans for a meeting in Paris to reorganize the Internation Society of Soil

We arrived the next morning and I called at the Embassy and was able to make arrangements for the Agricultural Attaché, Mr. Ben Thibodeaux, to have permission from the Ambassador to join our party a little later in southern France, which he did.

The French had made extreme claims of low grain yields in Algeria, claims that our people doubted. So I was requested to estimate the yields and report back at the Embassy.

Science.

At our meeting in Montpellier, Ogg was dead set against the plan of a Paris meeting. Somehow he had the idea that FAO was back of the reorganization of the Society. I couldn't do anything with him by explaining that FAO had nothing to do with it. He hated John B. Orr, the Director-General of FAO, and never spoke a kind word for FAO, as Orr never did for soil science. He scared out the other men from Britain except Walter Russell, who fortunately took my side. We were both much attracted to Edelman who returned to Holland for discussions with his government and to make plans to meet us in Paris with Professor Hissink. When Prof. Demolon heard that Hissink would be there he checked our because he wouldn't sit in the same room with him. Because of his close relations with Demolon, Aubert did not come, although he always supported the effort I made.

~~of his close relations with Demolon,~~ Aubert did not come, ~~although he always supported the effort I made.~~

We arrived in Algiers May 9 and had a fine field conference over much of the country with another paper-reading session at Oran. Between May 20 and 22 we went from Oran to Paris. It was a rough trip because of the heavy seas and the crowded boat and train.

Edelman, Zuur, and Hissink came down from the Netherlands and we had the meeting in Oudin's laboratory. As I recall there were about 8 or 9 of us. We agreed that we should start a new Society and that Hissink should take no more actions until the Society could be reconstituted in Amsterdam following our acceptance of the Dutch invitation. Thus the Congress in Amsterdam would have two independent parts: (1) The usual paper-reading sessions and field excursions and (2) sessions for constituting the International Society of Soil Science. After everything was signed and sealed and Walter Russell had returned to Britain, Hissink and Zuur came around to my hotel to get me to change it and declare Hissink the real Secretary-General. The old man was really frustrated. He was

very grateful to me for having cleared his name from the gossip spread by Demolon that he had been a German collaborator. On the other hand he was mad with me because I wouldn't approve his continuing as Secretary-General. He kept both of those unresolved feelings until he died. Hissink was an arrogant old Dutch professor. His hearing had gone bad and he had made many mistakes in judgment. He was not fit to be Secretary-General. I did not like him. But this is a far cry from calling him a German collaborator without evidence.

Happily the Dutch made Edelman chairman of their organizing committee and to try to avoid problems with Hissink, it was arranged for me to be chairman of the session dealing with the organization.

In summary I can say that this was an enormous undertaking. Especially difficult was this conference in Paris. The future of the Society hung almost on a thread because of the many jealousies.

After this business was concluded we ran into three straight French holidays. I was very glad for the rest, and especially my back was glad.

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Mr. Thibodeaux had brought back the soil samples that I had collected and opened them up to dry as I had suggested. Mrs. Thibodeaux said that this worried the servants a bit for fear that she would be starting some sort of garden. I packed them up and the Embassy sent them to my office.

I checked my confidential yield estimates. Fortunately I had gotten a good cross section of the grain lands and they checked the French estimates as given to our people.

On May 27 I went to Brussels. I got a good briefing with the officials of INEAC and my tickets. I left for the Congo May 30. (For the details of my journey and explorations, see the Congo Journal - 1947.) This turned out to be a very pleasant flight with a small plane like the one I rode in over Siberia. We started early each morning and flew non-stop and then had the afternoon and night in a hotel. We stopped in Tunis, Cairo, and Khartum.

In the afternoon of June 2 Mr. Jurion met me in Stanleyville. We looked at the great falls of the Congo River and had a pleasant evening. The next day we drove to Yangambi where I worked and studied until we began our long excursion June 6. I staid in a little cottage near Jurion's house and ate my meals with him and his assistant. Mme. Jurion and the children were in Belgium. Two Dutch from the Indies had asked to come to Yangambi. One of them was the arrogant head of a big trust. His chemist who came along was more subdued. They had their meals with us and they were continually criticizing the Americans for their silly talk about the four freedoms. Each evening at dinner Jurion would raise the question of the oil palm phosphorus problem and it would get discussed, but I would have to say I hadn't formulated a theory yet.

The last evening I was ready for dinner a trifle early and Jurion said, "Now Doctor, we leave tomorrow. What about the oil palm problem?"

I replied, "Today I am ready to propose an explanation to you. From some source the soils get sulphur added to them." (During

my visit in 1954 I found the source - sulphur-collecting vegetation in the tropical rain forest.) "The soils are highly leached here; the sulphur is changed to sulphuric acid by the micororganisms; and the magnesium goes out of the upland soils as magnesium sulphate. Your laboratory data show that the upland soils are very low in magnesium whereas it is abundant in the soils of the lowlands. Now to go to the plant, it can take in phosphate in several forms but it is moved within the plant as magnesium phosphate. This is especially important with plants having oily seeds, such as African oil palm. So your phosphorus deficiency symptoms are not so much due to a lack of available phosphorus in the soil as to a lack of magnesium to transport it within the plant. The phosphatic fertilizer used had a calcium carrier. Thus by raising the calcium-magnesium ratio in the soil the intake of magnesium was further depressed thus worsening the deficiency symptoms and lowering the yield. Now, of course, Jurion, this all needs to be tested by experiments.

Just then the Dutch came in. Jurion said to them, "Tonight the Doctor has an explanation for the oil palm problem."

The Dutch chemist asked him, "And what does the Doctor say?" Jurion went through the explanation that I had just recited. The chemist looked quickly at his boss, who nodded his head affirmatively. Then the chemist said, "The Doctor is right." (not "probably right" nor "possibly right" but "right".) Then he went on to explain in some detail the many experiments thay had performed to reach the same conclusion. In addition he called attention to the obvious need of watching also the balance between magnesium and potassium.

Jurion was a model of courtesy and suppressed his anger. These men had heard the discussion of this problem three or four times. They were leaving too the next day after accepting his hospitality. And yet they were going to say nothing.

In the morning we evened the scores a bit. The arrogant fellow showed me a beautiful brochure of his trust and pointed to a large photograph of their trust building in Batavia. He said, "You don't have anything so nice as that in New York or Boston."

I replied, "Of course we don't. It would be impossible to have a building like that in Boston."

He asked, "Why do you say impossible?"

"Because," I replied, "Your building in Batavia must be scrubbed every morning with the sweat of the Javanese and we don't have any."

The old boy was so mad that he couldn't think of anything to say until it was too late.

Jurion told this story many times and felt that we hadn't come out so badly.

Early on June 6 we started out on our trip with our baggage, extra tires, fuel and oil, and two natives, one as valet and one as driver. We went north to the station at Bambesa. At each station we visited everyone was delighted to see Jurion, from the youngest yard boy to the director. I soon began to appreciate that Jurion was the best research administrator I had ever known. Although he did not call himself an expert in any field, he could talk with the soil scientists, the plant pathologists, and all the rest.

As we went along, I became more and more impressed with the evidence for changing climates and a multiplicity of erosion cycles.

One day as we were driving along Jurion flipped a cigarette into the roadway. His watch band broke and the watch fell out with the cigarette. It had hit a little stone and simply fell into numerous pieces. These he gathered up into a matchbox and asked me to take back with me to Brussels so Mme. Jurion could get it repaired. I doubted that Mme Jurion could find such a skillfull watchmaker so I wrote home to Lucille and asked if she couldn't line one up that I could send him as a gift.

On this long trip to many stations Jurion was always receiving and sending telegrams. He learned not to bother me with the things for fear I might worry about our reservations. (Which I probably would have done!)

I never quite knew what the Belgians really thought of the native girls. They had a saying in Africa about native mistresses: The Belgians don't go in for it; the British take it for granted; the French think it is chic; and the Portugese just love it. At least in the Congo I saw very few mulattoes. But one night at the Mulungu Station during dinner where only gentlemen were present my hosts

spoke of one Belgian that had a native mistress. (In this area the native girls dress their hair with rancid butter.) This man required his mistress to take a shower and a drench of cologne. I remarked that I didn't see how a man could take up with a girl like that. Jurion spoke up and said. "Let me put you down here for about two or three years on one of the small lonely stations, Doctor, and it would improve your understanding enormously."

One of the most interesting discussions in the Congo Journal concerns the soils around Elisabethville, especially the excellent examples of Ground-Water-Laterite. During our visit there we were the guests ~~XXXXXXXXXXXXXX~~ of the Union Minière for lunch at the "Club". I sat next to the general manager, After considerable chit chat he said, "We are very glad to have you here to help us get a large experimental station going."

I asked him, "What do you expect to have done at this experimental station?"

"Well," he replied, hesitatingly, "To make experiments on these soils so that we can have plenty of food crops."

I said to him, "If you have an idea I shall do my best to work on it. But I wouldn't know where to start now except possibly for a small station to do some basic research on the nature of these soils. Then we might get something from that to give us some leads. I suspect that for a long time to come it will be more economical to produce the bulk of your food crops at some distance and ship them here than it would be to try to farm these soils."

As we walked away from the building, Jurion said, "Maybe they will believe you now. I've been telling them the same thing for years." A little later I reflected how Jurion had been very careful to have me see the soils before the luncheon was arranged without telling me in advance about the questions that he knew would arise.

Jurion had sent the car back to Yangambi from Costermansville and we had taken the plane to Elisabethville. Then he got a car that we drove to Kaniama where we visited a soil survey party under A. Focan. We had a meal with them. Both Focan and his wife loved to eat and showed it. On this trip, I became ill the night of June 27 in Kaniami, I had visions of dysentery but Jurion told me

not because if so I should be still worse. I had attacks of the illness until I returned home. My physician explained that the trouble was only extreme fatigue. We went on to Luluabourg. After visits there we flew to Leopoldville and arrived just in time to see the crowds ready to welcome Prince Regent Charles. (A different "Charles".) We called on the officials and saw a bit of the city.

For all his charm, Jurion disliked the military and diplomatic parties. He had a beautifully sarcastic way of saying, "le militaire" and "les elegance". And third he was angered to see a native drunk, and especially by the persons who sold liquor to the natives.

I stopped into the American Consulate to make arrangements for sending my extra gear back to my home office by pouch. The young man at the desk made the arrangements and then suggested that we should see the Consul - a Mr. Guynn. I was terrible embarrassed. Apparently he didn't know who Jurion was. During all our visit he gave us a tirade about the terrible service and inefficiencies of Belgian administration in Leopoldville, much of

which I knew from experience to be untrue. He was simply ridiculous. (I reported this incident when I returned and he was reassigned.) As we came down the steps of the Consulate, Jurion put his arm over my shoulder and said only, "Don't worry, I suppose we have some of the same kind."

In Leopoldville Jurion and I had long talks about the research programs of the INEAC. One evening, at a nice restaurant overlooking the Congo River, I said, "You know, Jurion, this would be a wonderful place to have an International Soil Congress."

"They would never come here so far away," he replied.

But then we went on and discussed it. The British had started many schemes in Africa and the American Marshall Plan had just been announced. We could expect a greatly increased interest in the tropics. I asked him if he thought his government would approve if I could stir up the interest. He wasn't sure. "It's a dream," he said. And it turned out to be a wonderful dream that came true in 1954.

We also talked about the ultimate status of the Congo. I suggested a scheme like that for the Philippines in which the United States set a specific day for their independence long in advance. He thought it was too early to think about it.

Unhappily, Jurion had to return to Yangambi in a day or so in order to receive the Prince Regent and other dignitaries. So I was turned over to Mr. C. Donis, a forester in INEAC who took me into the Low Congo.

July 1 I was again ill but Donis and I took a little holiday across the river to Brazzaville. That evening we went to dinner with Jurion and I saw some little ivory elephants that I wanted. Jurion asked me what I would be willing to pay for them and then he and the Arab merchant began bargaining, each roaring with sarcastic laughter at the other's suggested price. This was one of the funniest incidents of the whole trip, and in a few minutes Jurion had them at the price I suggested. I don't know what would have happened at a lower price - probably only louder laughter over a longer period.

July 2 I said a sad farewell to Jurion. My admiration for him was boundless (and it hasn't changed over the years.) And Donis and I took our very interesting excursion into the Low Congo.

We returned to Leopoldville July 8 and I gave a radio interview in English about INEAC and the place of food production in world peace.

I was supposed to leave early the next day and went out to the airport on schedule. But Sabena had decided not to fly that day. Fortunately I had just gotten developed photographs and spent all day on them.

I left the next morning, July 10 and flew with brief stops to Brussels in about 26 hours. In those days Sabena had no competition and the service was poor. I went immediately to the American Embassy and then to an exchange bank. Then I called at the INEAC headquarters about 4:00 p.m. with my bags hoping to see when I should give my report to them. Instead, they ushered me directly into a meeting of the Board of INEAC and asked for my report immediately. Never was I more tired and, of course, I had

no notes. I don't remember what I told them but it must have been all right. I recall only two exchanges:

I spoke of the serious erosion in the Albert National Park resulting from the removal of the natives. They had annually burned the savana and the new growth attracted the antelope. After the natives were removed and fires prohibited, the antelope left the Park, and along with them went the lions who are the natural predators of the hippopotami. These had increased so enormously that the overgrazing was stimulating erosion and would produce an unnatural landscape for the area. Thus the hippopotomi numbers would decline eventually through starvation. I urged that they be reduced now through trapping or shooting. Prof. van Stralen nearly hit the ceiling. "There will be no shooting of hippopotomi!" From this argument I learned never to go directly against the professor but always to begin with a point of his such as, "Now following up your point, Professor ....." With that method you could lead him anywhere as I often did later.

And then I recall that Mr. LeBrun asked me in stumbling English, "How did you talk with Jurion if you don't speak French well?"

I replied that we spoke English. But LeBrun insisted that Jurion couldn't speak English. When I told him that Jurion could lecture in English if he chose to do so, he could hardly believe me. After this session, I went to my room and directly to bed without dinner.

Before leaving Brussels I had dinner chez Jurion. I discovered that Mme Jurion was pretty, vivacious, and wealthy. Later I wrote to Jurion that this was too much on top of all his other attributes and gifts. At least he could have had a poor and homely wife.

With much difficulty I got a plane to London on July 13. (My excursion in the United Kingdom is covered by Mediterranean Journal, 1947.)

I staid at Rothamsted chez Dr. A. Muir for four interesting days and then went to Bangor, Wales where I staid with Prof. G. W. Robinson and had some very interesting field trips that

are fully covered by the Journal.

But one incident may be recalled. Wright of New Zealand was also in town. And we had a couple of evenings with him. He spoke of the kauri tree and egg-cup Podzols under them. (See Down Under Journal, 1949.) These trees had a most dramatic influence on the soil. Up to then Robinson had never quite believed either Dr. Marbut or me on the great direct influence of vegetation. A bit after that I went to hear Robinson, and others, lecture at a meeting of ecologists. The audience sat on the most uncomfortable student benches I have ever seen. In the middle of his prepared talk, Robinson said that vegetation had little direct influence on the soil, Then he paused and said, "Recent information that has come to me since I prepared this paper forces me to say that under some conditions vegetation has a very great influence on the soil." Of course it does ~~xxxxxxx~~ everywhere but still Robinson had come a long way.

July 21 I went to London and had difficulty getting a room. On the next day I took a boat train to Paris and finally got to

the Gare des Invalides. TWA told me that the plane would soon be ready to go, but it didn't. I staid there until 4:30 AM and we left a little after 6:00 a.m. Fortunately Lucille met me at the airport in New York with tickets for home.

After a few days I was back to normal and almost gloated over the wonderful education I had had, especially with Jurion.

For one thing Stefferud was putting together the 1948 Yearbook - Grass. I asked him how it was going. "It is monotonous," he replied, "everyone says what a wonderful thing grass is." But I explained that it usually is not in the tropice; it degrades the soils. He insisted that I had to write a piece about this. He gave me only a few days to write the little article Grass and the soil for that Yearbook. Then a little later I wrote a piece on shifting cultivation for a bulletin on "Soil Conservation" published by FAO.

Soon after I returned a questionaire came from a young woman doing a degree thesis on "regions". She sent it to all who had regional maps on file in the Civil Service Commission. I then

realized that people might regard my inspection regions as administrative regions. I decided to get rid of the word "regional" because the Congress was already thinking of reducing such officies. But what to do about the word "inspector". For years the inspectors had complained about their titles but ~~have~~ had been unable to make an alternative suggestion. So I listed all their duties on a sheet of paper. Soil correlation was the big one. In the dictionary I found the verb "to correlate" but no noun " correlator" as "one who correlates". One is not supposed to use words that are neither in the government Style Manual of Webster's big dictionary. Still I doubted whether the personnel clerks up the line would check either, and they did not. So on September 15, 1947 I sent out the changes in title like the following, "Regional Inspector, southern region" became "Principal soil correlator, southern States" followed by the names of the States. The titles of the next in line were changed from "inspector" to "senior correlator". (The next year the Congress did make a cut in regional offices and this change in titles saved us over $10,000!)

In September I was writing to Dr. Black and trying to push the idea of pilot-research farms. Unhappily, I was unable to get many to understand. These farms were for pushing interaction reaction research to the limit on a whole farm basis.

Dr. A. H. Moseman was looking for a lot on which to build a house. I asked him that if he found a wooded one of one-half to one acre, I should like to see it. He did find a nice one for $1,200 and I agreed later to buy it, with the money I had coming from Macmillan for the royalties on the USARI edition of The soils that support us. Later I made maps of the trees and harvested surface soil for gardening at home. But my back got worse and I gave up the idea of a grand azalea garden. Finally I sold the lot for a bit over twice what I had paid.

In September or October I got a strong letter from the editor of the "Scientific Monthly" about the piece on soil doctrines. I went to work on it. Apparently my subconscious mind had been busy because it came fairly easily. I took the ballet as a model. The background theme told the basic principle of soil science while I

highlighted seven special themes: The balance-sheet theory; the erosion menace; the organic schools; earthworms; plowless farming; "balance" minerals; and soil and health.

I wrote this very carefully and as simply as I knew how. I sent copies to several for review. Then finally I got it off to the Scientific Monthly near the end of the year.

In October, Hearn retired. He was the last of the old inspectors to leave.

I spent November 6 and 7 at Lake Success in New York with a planning session for a big conference on resources that was to come in 1949.

The Society meetings were in Cincinnati from November 17 to 20. I went early in order to arrange a special meeting of those selected to take part in a special issue of "Soil Science". Dr. Firman E. Bear had suggested that I take one issue, select whom I wanted, and bring the soil classification part of the 1938 Yearbook, Soils and Men, up to date. This took an enormous amount of work during the next year and six weeks but we got a good volume that had much influence over the world.

Then too, I gave a full report on the meetings in Paris and the Dutch invitation for a Congress of the International Society of Soil Science in 1950. Most of the remainder of my time was spent with individual conferences about men I wanted or that other people wanted and about the many cooperative projects under way.

Early in December I spent a few days with TVA in Knoxville on policy matters, especially test-demonstration farms. I agreed to spend several weeks with with them the next year if possible.

FAO had asked the American Society of Agronomy to appoint an advisory committee on fertilizers to help FAO prepare an international book or bulletin in this field. I was made chairman and had a hand in selecting the other members. This was also a big job and Dr. Ignatieff and I worked closely together on it for a year or so. This came out as Efficient use of fertilizers, the first really international work in the field that could be understood by ministers of agriculture. Besides Spanish and French editions, all or parts were translated into other languages.

By now I was having a large correspondence about agricultural policy with both Dr. Black and Dr. Hardin, as applied to the Department, the colleges, and TVA.

In December I received a severe shock. I was sent a copy of a terribly scurrilous bulletin written by, of all people, Richard Bradfield. It was an illogical, badly written tirade against the National Soil Fertility Acts introduced into the Congress as H.R. 3421 and S. 1251. The bulletin was called Should fertilizer production be subsidized? and published by a right-wing outfit called the American Enterprise Association that had somehow obtained some good names on their "Advisory Board."

I wrote a letter about this to Dr. Cline. He had never heard of it. In fact I never found anyone on the soil science staff at Cornell that ever did see the thing to this day. Had Bradfield let them review the manuscript, may gross errors in soil science could at least have been avoided. The whole bulletin of 42 pages goes contrary to the well-known data (even to some in the bulletin itself!) and to the public and private statements of the men supporting the program.

Bradfield somehow got sucked into this job by somebody with an axe to grind and dashed it off. I know that later he was ashamed of it. Very few copies were distributed so I suspect that he withdrew it and paid back his fee, but I don't know. The following summer at Fort Collins, George Scarseth, Bradfield, and I were talking together about some point about fertilizers. George, speaking to Bradfield, quoted some phrase from this bulletin. Bradfield's face turned red as fire and he looked at me. Neither then nor at any other time have I said anything about it to him. Bradfield commonly wrote ~~bad~~ some poor papers. He would promise to write one, delay until the last moment, and then dash it off without any criticism. But this one was way below anything else that ever got printed. What a great pity it was.